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ART. XXXV.—*On Photographs of the Spectrum of the Nebula in Orion;* by HENRY DRAPER, M.D.*

FOR about eighteen months I have been giving attention to the Nebula in Orion with two objects in view, first to ascertain whether any changes are taking place in that body by making a series of photographs to be compared in the future with a similar series; and second, to photograph the spectrum of the Nebula in various parts so as to see whether any new lines could be found, and also whether the composition is uniform throughout.

As to the first of these objects I have recently succeeded in taking a very fine and extensive photograph of the Nebula containing most of the delicate outlying parts which were not in my earlier photographs. This is in the hands of the photo-lithographer now and will shortly be published. The experiments have been very difficult because an exposure of more than two hours in the telescope has been necessary, and an exceedingly minute motion of the stars relative to the sensitive plate will become apparent on account of the high magnifying power (180), employed.

In carrying out the second object two contrivances have been used; first, a direct vision prism in the cone of rays from the objective before they had reached a focus, and second the two-prism spectroscope with which I have taken photographs of stellar spectra for some years past.

* Read before the National Academy of Sciences, April, 1882, at Washington.

During the month of March I have made two good photographs with each of these arrangements. Those with the direct vision prism, without a slit, have of course demanded that the image should be kept stationary on the sensitive plate throughout the exposure, viz., two hours, and they are as difficult to get as good photographs of the nebula itself. On the contrary, those obtained with the slit spectroscope do not require the same steadfast attention.

The results derived from these photographs are interesting partly from what they show and partly from what they promise in the future. A number of photographs, under various conditions, will be needed for the full elucidation of the subject.

The most striking feature is perhaps the discovery of two condensed portions of the nebula just preceding the trapezium, which give a continuous spectrum. At those places there is either gas under great pressure or liquid or solid. I have not been able to detect any stars of sufficient magnitude in these portions to produce this effect either in my photographs of the nebula or in any of the well known drawings of this object. It seems to me also that the photographs show evidence of continuous spectrum in other parts of the nebula. In these respects the conclusions arrived at by Lord Rosse in his memoir (Phil. Trans. Royal Society, June 20, 1867, page 70), are to a certain extent borne out.

The hydrogen line near G, wave-length 4340, is strong and sharply defined; that at *h*, wave-length 4101, is more delicate, and there are faint traces of other lines in the violet. Among these lines there is one point of difference, especially well shown in a photograph where the slit was placed in a north and south direction across the trapezium; the Hγ line, λ 4340, is of the same length as the slit and where it intersects the spectrum of the trapezium stars a duplication of effect is visible. If this is not due to flickering motion in the atmosphere it would indicate that hydrogen gas was present even between the eye and the trapezium. I think the same is true of the Hδ line, λ 4101. But in the case of two other faint lines in this vicinity I think the lines are not of the length of the slit, one being quite short and the other discontinuous. If this observation should be confirmed by future photographs of greater strength it might point to a non-homogeneous constitution of the nebula though differences of intrinsic brightness would require to be eliminated.

The April number of the American Journal of Science contains an account of a photograph of the spectrum of this nebula taken by Dr. Huggins. I have not found the line at λ 3730, of which he speaks, though I have other lines which he does not appear to have photographed. This may be due

to the fact that he had placed his slit on a different region of the nebula or to his employment of a reflector and Iceland spar prism, or to the use of a different sensitive preparation. Nevertheless, my reference spectrum extends beyond the region in question.

As illustrating the delicacy of working required in this research it may be mentioned that in one of these photographs the spectrum of a star of the tenth magnitude is easily discerned. It is only a short time since it was considered a feat to get the image of a ninth magnitude star, and now the light of a star of one magnitude less may be photographed even when dispersed into a spectrum.

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ART. XXXVI.—*Mean Annual Rain-fall for different Countries of the Globe;* by Dr. ALEXANDER WOEIKOF of St. Petersburg, Russia.

PROFESSOR E. LOOMIS having published in this Journal a paper on the rain-fall of the world (Contributions to Meteorology, paper xvi), inviting meteorologists to give supplementary information on this subject, I readily accept this invitation, having myself given much attention to the subject. I must remark first on the exceeding inaccuracy of the geographical delineations used on the map, (mountains, lakes, rivers, etc.,) an inaccuracy which, however, certainly did not mislead Professor Loomis, when he was drawing his lines of equal rain-fall for countries where observations are wanting, and where the geographical configuration and notices of travelers are the only objects which give a basis to the hypotheses. I will give only examples in relation to Africa and Asia. On the first continent not only are none of the equatorial lakes given, but even no mountains in Abyssinia, while we find "Donga Mountains," between 4°–6° N. and 20°–30° E., which have no existence. In Asia the Stanovoi watershed between the systems of the Lena and Amur is given as if it was a high mountain chain; the same is to be said of the slope of the Mongolian plateau north of Pekin, while between 25°–40° N. and 96°–110° E., no mountains are shown, while this region comprises West China, Kukunor and Eastern Thibet, and has some of the highest chains of Asia.

I come now to Professor Loomis's work in Europe. The shades are mostly right, except—

1. Sicily, which has, in its greatest part less than 25″.
2. Portugal, where the author cites the old and erroneous

figures for Coimbra, while Professor Hann has some years ago proved, by the results of the new observations, that it has a rain-fall not much above that of Lisbon.

3. Scandinavia, where the fall above 50″ is restricted to a few exceptional places, near high mountains of West Norway, like Bergen and Floroë. Even Christiansund has much less. Eastern Norway and the interior valleys (Christiania, Dovre) have less than 25″.

In what concerns European Russia, the shading is perfectly right, as I have found by a new and more extensive collection of data. The frontier of the fall of more and less than 10″ is also in the main right, except in the vicinity of Orenburg, where it should be drawn somewhat more to the south.

In Asia, north of the Tropics, there is more to change. Thus the zone of less than 10″ in the Arabo-Caspian steppes and vicinity is too large in Professor Loomis's map. It does not stretch so far to the north and northeast, and certainly does not reach to the foot of the Altai, where the rains of summer and the snows of winter are rather abundant. On the east coast of the Caspian, Krasnovodsk (40° N.) has also more than 10″.

There is a great rain-fall on the south shore of the Caspian, as well as on the south part of the western shore: on the latter Professor Loomis has the station of Lenkoran 51·7″.

The country about Lake Balkash and the upper Irtysh is very dry, it being really a continuation of the Arabo-Caspian steppes, there being a broad gap in the mountains. The dry region in Mongolia and Eastern Turkestan must be extended westward so as to include Yarkand and Kashgar; on the north, on the contrary, it does not extend as far as shown by Professor Loomis, Kiakta and Ourga having more than 10″. The good pasture and forest in this region prove that the two latter stations are not exceptional. I should rather include most of the basin of the Yang-tze and eastern Indo-China in the region having more than 50″, though I have no observations to prove my case. But the known humidity of the region, the luxurious vegetation, the immense floods of the rivers* corroborate my opinion. Eastern Java seems to have less than 75″, as is seen by the 5½ years of Surabaya and 2 years, 1879–80. It would be safe to give 50″–75″ to the islands east of Java so far as Sumbawa, and a shade less to Flores and Timor, while Banca, Billiton and the Moluccas and Southern Philippines have probably over 75″.

As to British India, Professor Loomis has made a good use of the numerous observations there.

* See E. L. Oxenham, Inundations of the Yang-tze-Kiang, in Jour. Roy. Geogr. Soc., 1875, p. 170.

In Western Asia the region of 10″–25″ is probably in closer proximity to mountains than shown in Professor Loomis's map, extending far to Shiras along the mountains of southern Persia, and not so far to the south in southeast Syria and Mesopotamia. In Arabia a closer approximation of the rainy regions to the mountains is also what must be the case.

As to Africa, Professor Loomis has made good use of the observations and general information of travelers, with the exception of Abyssinia, which has certainly more rain than indicated by him. That North America north of the Tropic is accurately represented by Professor Loomis is easily understood. I would only ask, is it not probable that Northern Canada from about 50° N. lat. is less rainy than supposed by Professor Loomis, so that the more abundant rain of the coast of Labrador would be caused by its position relatively to the sea and mountains?

As to Central America, the dark shade given to Yucatan is rather doubtful. The short period of rain, the absence of mountains and the rather scanty vegetation, at least of Northern and Western Yucatan would rather point to less rain, though the porous nature of the limestone absorbs much water. So far as I could see and learn, Tehuantepec and Northern Yucatan must have about the same quantity of rain.

As to the Pacific coast, from about Tonala to Panama it must have above 50″. A few observations made in Nicaragua give a large amount of rain, and Nicaragua is certainly less rainy than the coast of Soconusco about Tapachula and Guatemala. The city of Guatemala has a little less than 50″, but the plateau on which it is situated is generally dryer.

Tropical South America, at least its interior, is the part of Professor Loomis's map which is least accurate. I am at a loss to know on what account the Amazonian country is supposed to have less rain than the dry "llanos" of Venezuela to the north of it. I consider it probable, that from the north coast to 15° S. lat., the only parts east of the Andes, where there is less than 50″ rain, are the llanos of Venezuela and perhaps part of the Magdalena basin. On or near the Amazon, we have Para, Manaos and Iquitos, which give more than 50″. The latter is especially interesting. The distance from the Andes is more than 200 miles, from the Atlantic more than 1000, the country perfectly level, and yet there are 111·8″ rain. Generally it is mentioned by travelers, that to the south of the Amazon the rains are less heavy, and yet San Antonio on the Madeira has 91·3″.

As to the west coast, the region of more than 75″ extends certainly to the equator, the provinces of Esmeraldas and Choco being described as exceedingly rainy. Farther to the south I would object to giving to Chiloe and the adjacent coast

less rain than to Concepcion and Valdivia. Professor Loomis probably made use of the observations at Ancud, Chiloe, and seeing that this place had less rain than Valdivia and Puerto Montt, took it as generally admissible for the latitudes above 41° S. on the west coast. But Ancud is protected by a rocky promontory on the west, that is, the wind and rain side, while the other two stations are open to the west.

In the LaPlata States the interior is very dry, only the eastern slopes of the mountains and their vicinity have more rain. Now all the interior stations from which we have rain observations, are so situated, and all except Mendoza give much more rain than would be the average of more evenly distributed stations. We know that westward from the stations of Cordoba, Tucuman, Salta are the States of Catamarca, Jujuy, San Luis, which are so dry that even good pasturage is scarce. It would thus be safe to extend the region with less than 10″ much to the eastward, and that of 10″–25″ also.

As to Australia, the region of 25″–50″ in the north of the Continent probably extends along the coast somewhat to the southwest from the position given it by Professor Loomis. As to that of less than 10″ there is no reason why we should admit that it is so very restricted in the interior as in Professor Loomis' map. We have no observations from an extensive district, but what we know of the dryness of the interior allows us to make this very probable supposition.

On the whole, Professor Loomis's work is certainly a good one, but on account of what I have mentioned above, a revised edition of the map is yet desirable.

I would besides draw attention to some perplexing and erroneous designations of the countries and regions in which are situated some of the stations mentioned by Professor Loomis. I give here some of them, with the corresponding numbers of Professor Loomis's table.

254 *Ragusa*, European Turkey. Is situated in the Austrian province of Dalmatia.

363 *Ajansk*, Russia. As other places in Siberia are cited as such, it would be better to mention Ajan, the exact name, as being in Eastern Siberia. The same applies to 524 *Nertschinsk*, 522 *Irkuzk;* 646 *Kjachta*, 510 *Enisseisk;* while 512 *Tobolsk*, 516 *Tomsk*, 517 *Ischim*, 519 *Omsk;* 520 *Isalair* (*Salair*), 643 *Barnaoul*, 644 *Akmolinsk* are situated in Western Siberia.

521 *Nikolaiewsk*, 525 *Blagowestcheuck*, 526 *Chabarawka*, 528 *Vladivostok* are placed by Professor Loomis in China, while they are in Eastern Siberia (in the Amur and Littoral provinces).

The appellation Tartary applied to 529 *Tashkent*, and 647 *Irgis* is wanting in precision. If it is meant for the Kirghiz

steppes, Russian Central Asia and the Khanates of Khiva and Bokhara, then the following stations should also be included in it: 649 *Raïmsk*, 650 *Fort N.* 1, 652 *Novo Petrovsk*, 653 *Nakust Mekuss*, 654 *Petro-Alexandrovsk*.

I give below some data about rain-fall, especially in relation to countries which, in my opinion, are not correctly represented in Professor Loomis's map.

Names of Places.	Lat.*	Long.*	Ht. met'rs	No. of Years	Rain. Inch's.	Authority.
Manaos, Brazil	−3° 8′	−60°	37	1	55·24	Zeitschr. v. 8, p. 267
Iquitos, Peru	−3 44	−73° 8′	95	1	111·8	
S. Antonio, Madeira, Brazil	−9 5	−64	....	1	91·3	Zeitschr., v. 15, p. 492
Pernambuco, Brazil	−8 4	−34 52	3	4	108·4	Zeitschr., v. 14, p. 216
Surabaya,	−7 13	112 48	....	5½	71·7	Zeitschr., v. 8, p. 56
Passœrœan,	−7 38	112 56	4	2	63·1	Bergsma Z. 1879 and 1880
Probolingo, Eastern	−7 44	113 13	10	2	57·33	
Besœki, Java.	−7 43	113 41	2	2	57·76	
Sitœbondo,	−7 41	114 2	80	2	57·05	
Banjœwang.	−8 13	114 23	5	2	68·83	
Amboina, Moluccas	−3 42	128 10	....	2	190·50	
Banda,	−4 32	129 53	....	2	135·77	
Urga, N. Mongolia	47 55	103 50	....	6	10·13	Met. Annalen
Kiakhta, S. E. Siberia	50 21	106 25	....	4	10·09	
Irkutsk,	52 17	104 22	....	6	17·38	
Barnaul,	53 20	83 57	....	41	9·50	
Akmolinsk, S. W.	51 12	77 23	....	6½	9·53	
Semipolatinsk, Siberia	50 24	80 13	....	5	8·39	
Salair (Altaï),	54 15	85 47	346	4	13·0	
Vladikavkas, N. Caucasus	43 0	45 3	....	8	34·04	
Stavropol	45 5	41 53	....	13	27·74	
Poti, E. co't of Bl'k Sea.	42 10	41 40	....	10	65·92	
Suchum-Kale,	42 58	40 53	....	3½	50·51	
Dachovsky-Possad Trans Cau-casia	43 34	39 42	....	9	80·81	
Novorossissa,	44 43	37 46	....	4	30·02	
Sevastopol, Crimea	44 36	33 32	....	14	15·37	
Christiania, S. Norway	59 55	10 43	...	7	21·18	Zeitschr. v. 4, p. 508
Sandösund,	59 5	10 28	....	7	23·01	
Christiansund, W.	63 7	7 45	....	7	33·18	
Aalesund, co't	62 29	6 9	....	7	45·53	
Catania (E.),	37 24	15 3	.. .	12	18·05	Fischer, Klima der Mittelmeerländer Gotha, 1879
Syracuse (E.),	37 4	15 17	....	10	18·27	
Palermo (N.), Sicily	38 7	13 19	....	61	23·27	
Sciacca (S. W.).	37 30	13 5	....	10	22·26	
Oporto, Portugal	41 9	−8 36	....	10	56·34	
Coimbra, Portugal	40 13	−8 25	....	6	34·0	
Lissabon, Portugal	38 43	−9 10	....	20	28·80	

* The sign − stands for lat. S. and long. W.

† The observations were continued only ten months and gave 49·9″, the two remaining, July and August are interpolated.

Art. XXXVII.—*Notes on Physiological Optics;* No. IV; by W. LeConte Stevens.

1. On Voluntary Control of Focal Accommodation.

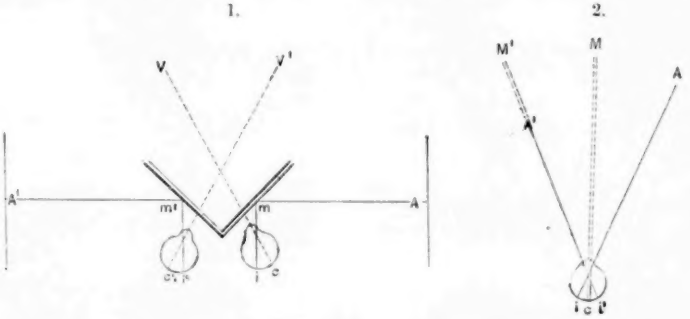
In my last paper the visual effect of associated muscular action of the two eyes was discussed, the investigation having led to the discovery of a new mode of stereoscopy. Experiments have also been made on the effect of muscular action in a single eye.

If the axial adjustment and visual angle be kept as nearly as possible unchanged while the gaze is directed upon a luminous surface, such as the globe of a gas lamp, it is very easy to throw the crystalline lens of each eye out of focus, accommodating for a nearer point, so that the object appears blurred in consequence of the production of diffusion circles on the retinas. I find it possible in this way to exercise extreme contraction of the ciliary muscle at will. It is impossible to avoid slight associated contraction of the internal rectus muscles, but this may be controlled to such an extent that the only effect noticeable is unsteadiness of one eye, while the other is directed to some previously clearly defined object. The effect is first an encroachment of diffusion circles upon the surrounding retinal area as well as upon the image itself, producing dimness of vision and hence apparent enlargement of the object which is necessarily ill-defined. But as the ciliary contraction increases, the object apparently diminishes in size to a marked extent, sometimes almost to half its previous area, while the halo due to diffusion circles does not widen, but on the contrary grows narrower. This is due to the contraction of the pupil, the effect being that of using a new stop in front of the lens, as it thickens and acquires more "depth of focus," while its theoretic focal length is less. The pupillary contraction can be noted by an assistant and approximate measures be made of the change in diameter of the opening. This pupillary change very quickly follows the contraction of the ciliary muscle. The area of my pupil has thus been repeatedly diminished, within two seconds of time, from 12 sq. mm. to 1·2 sq. mm. The estimated distance of the object is very uncertain. It may be judged nearer because of the sense of muscular contraction, or more remote, because of the dimness due to imperfect focalization. The only perfectly clear perception therefore, is that of marked dedecrease in area, far greater than can be referred to mere encoachment of diffusion circles. The distance between retina and lens remains sensibly constant, hence the perception is an illusion due mainly to abnormal muscular conditions.

Since in normal vision the contractions of the ciliary and internal rectus muscles are associated, the considerations just expressed show why so acute an observer as Wheatstone should have noticed the apparent decrease in size of the binocular image when strong convergence of visual lines was induced by pulling forward the arms of his reflecting stereoscope,* but failed to note the corresponding variation in apparent distance, though he observes that the image seems changed in position, but does not say whether the change is that of increase or decrease in distance.

2. Effect of Muscular Effort on Retinal Sensitiveness.

The estimation of size and distance, when there is strong, muscular effort, depends in some measure upon the part of the retina on which the image is formed, variations being more noticeable when the central part is impressed. In a former paper† I briefly noted the following experiment with Wheatstone's stereoscope. Let A and A′ (fig. 1), be a pair of conjugate pictures adjusted so that the reflected rays, $m\,i$ and $m'\,i'$, are sensibly parallel. If the visual lines of the eyes receiving these rays coincide with them, the image appears in front, combined in full relief, with apparent diameter and distance slightly magnified, the contraction of the internal rectus muscles being less than normal. Let the visual lines now be forcibly crossed. The retinal images, i and i', are no longer upon corresponding



retinal points: the external projections of them hence appear separate and without relief. For the subjective binocular eye (fig. 2) the two visual lines are combined into the median line cM. By indirect vision, the image of A appears on the right of the median, and that of A′ on the left, each diminished in size on account of the disturbance of ordinary retinal sensation by strong muscular contraction. This apparent diminution is greater than

* Philosophical Magazine, 1852, p. 508. † This Journal, Dec., 1881, p. 450.

can be explained by encroachment of diffusion circles at the edge of the retinal image, or on the supposition that the antero-posterior diameter of the eyeball has been slightly diminished by pressure on opposite sides of its elastic sclerotic coat. For reasons already given, the apparent distance is not very determinate. Without changing the convergence, let the eyes be rolled to the left until A′ is seen with the left eye in the direction of its visual lines. The image of A, seen still more obliquely, now appears slightly larger and dimmer, while A′ is still smaller. The apparent variation in size is thus independent of binocular combination.

3.

In such experiments one great advantage attained by using Wheatstone's stereoscope is, that by keeping the plane of each picture perpendicular to the arm that carries it, any distortion of perspective that might be due to changing the direction of the visual lines is reduced to a minimum. Making allowance for such disturbance, the visual effects just described may be obtained by means of fig. 3. A pair of circles, 2^{cm} or 3^{cm} in diameter are drawn 8^{cm} or 10^{cm} apart, their centers, A and A′, being similarly marked. Below A′ is another circle A″, equal in size. By cross-vision the images of A and A′ are combined, forming a diminished ellipse, each circle being seen obliquely: the minor axis is parallel to the interocular line. The monocular images on the two sides are sensibly circular, and larger than the central image, but smaller than either given circle as seen by normal vision. Even though distinct vision be attained under these conditions, the ciliary muscles are in a state of strong involuntary contraction. On closing one eye, these muscles become relaxed, while the direction of the visual line for the eye remaining open is easily kept unchanged. The elliptic image then apparently recedes, and in doing so it grows larger; but an interval of one or two seconds may elapse before normal monocular vision is restored. This process can be

reversed and made as gradual as is desired by slowly increasing the convergence of visual lines and carrying it beyond the point necessary for mere binocular combination. If the pair of circles just mentioned be employed, as soon as double vision is produced, the two interior images are perceived to be smaller than the others. They continue to grow smaller and apparently nearer to the observer until they coincide in the middle, and the diminution continues after they have been made to pass each other. In fig. 3, these phenomena may be noticed if the reader will combine the images by cross vision, placing the face as near as possible to the page. The angle of convergence should be at least 60°. The elliptic monocular image of A″ is perceived as suspended in mid-air. Its apparent position is not quite so nearly determinate as that of the binocular image above it, but it appears about as small and near. The left eye image, seen by indirect vision, has a much greater apparent area than the right eye image seen by direct vision.

In connection with the phenomena of optic divergence, this series of experiments proves that not only is convergence of visual lines not necessary for binocular vision and for the localization of objects in the field of view, but binocular vision itself is not necessary for the production of change in such localization. The mental impression received from the binocular image is clearer and the resulting judgment is much more definite, but no geometric method of determining the place of the external image is capable of covering the facts.

The experiments described indicate a closer connection between the nerves of the ciliary muscle and those of the retina than has been commonly supposed. Through the sympathetic system of nerves the mere action of light on the retina excites reflex muscular contraction of the iris. Without the agency of light, this may be effected by ciliary contraction alone, which is always accompanied with diminution in size of the pupil. It is by no means impossible that, through the sympathetic system, ciliary contraction should modify also the impression conveyed to the brain from the retina, while the size of the actual retinal image is unchanged. The interpretation is dependent upon the excitement of several different nerves; the thing interpreted is the product of forces from without that operate in accordance with well known mathematical laws. Helmholtz has determined approximately the velocity of propagation of a nerve impression, but beyond this there has been little success in bringing the physiology of sensation within the domain of mathematical law.

3. Relation of Axial Adjustment to Focal Accommodation.

The focal and axial adjustments of the eyes being necessarily dissociated to some extent, if clear vision is had with the stereoscope, but this dissociation being abnormal, it follows that the range of focal accommodation possible for a given pair of eyes should not be the same for different values of the optic angle. To ascertain the degree of disturbance thus induced, a series of measurements has been made with the assistance of Dr. J. H. Shorter, of the New York Eye and Ear Infirmary. In the preliminary examination it was found that at the distance of 9^m it was possible to read Snellen's test-type intended for average vision at 6^m. By the usual formula, therefore, my acuity of vision is $\frac{2}{3}$. With visual lines approximately parallel, there is hyperopia to an extent not exceeding one dioptric.* This is capable of correction, therefore, with a bi-convex lens whose focal length is 1^m. But when the visual lines are diverged $5°$, latent hyperopia to the extent of nearly one dioptric more is revealed by the use of appropriate test-lenses, while the near-point of distinct vision is found, by means of test-type, to have receded proportionally. This fact could not well have been ascertained otherwise, except by the use of atropine, with its well known disagreeable consequences. The experiment shows that, in the present case at least, full relaxation of the ciliary muscles is not attained by making the visual lines parallel. For emmetropic eyes it is usually assumed that the passive condition is that of parallelism. The theory of negative accommodation advanced by Von Graefe† and Weber has not been generally accepted.

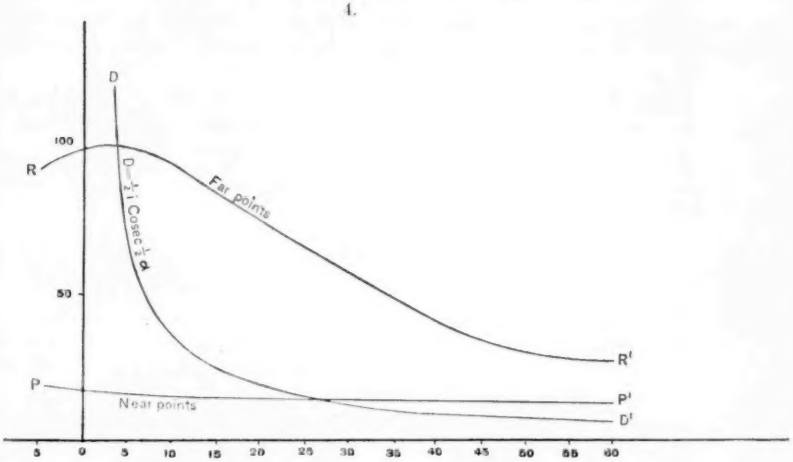
In experimenting upon range of accommodation I employed a pair of narrow cards on which were vertically printed similar specimens of test-type, intended to be read with average acuity of vision at the distance of 67^{cm}. On a large sheet of paper, covering a table, a median line was drawn, terminating at the edge in front of the mid-point between the eyes. Making due allowance for the distance of these from the edge, pairs of lines were drawn, along which the visual lines could be directed at will. Upon them the cards were placed so that binocular fusion of images could be secured for fixed values of the optic angle, to which the directions of the lines had been adapted. Along these the cards were made to recede and approach until the farthest and nearest points were found at which distinct binocular vision was possible, myself being kept ignorant of the record taken until the whole series of measurements had been completed.

* For dioptric system of measuring the refraction of the eye, see Carter on Eyesight, pp. 43–47; Macmillan, 1880.

† Soelburg Wells, Diseases of the Eye; Appleton, 1881.

The result is shown in the upper and lower curves of fig. 4, in which distances are expressed in centimeters, for successive values of the optic angle, taken as abscissas: RR′ is the curve of far-points, PP′ that of near-points, and DD′ that of distances of the optic vertex determined by the intersection of visual lines. The latter curve crosses PP′ near the point corresponding to $\alpha = 25°$, showing that, for values of the optic angle greater than this, distinct vision of an object as near as the optic vertex is impossible. This limiting value of the optic angle becomes still smaller with increasing age. The range of available accommodation in the present case is seen to be greatest when $\alpha = 3° 30'$. This is about the angle enclosed by the visual lines when a single point is seen at the distance of 1^m.

4.



At $\alpha = -5°$, the increased dilatation of the pupils, which accompanies relaxation of the ciliary muscles, reduces acuity of vision slightly, while the corresponding flattening of the crystalline lens causes recession of the near-point. The available accommodation is hence diminished, while the total accommodation remains unchanged, the portion lost by hyperopia being thus not available when convex glasses are not used. As the optic angle increases beyond 3° 30′, the distance of the far-point rapidly diminishes, so that, at $\alpha = 60°$, more than three dioptrics of temporary myopia have been induced. It was found that at $\alpha = 4°$, the weakest convex lens impaired vision; this amount of convergence is hence sufficient to completely mask my hyperopia, stronger convergence much more than masks it, parallelism makes it manifest, while divergence reveals that which is latent

and not otherwise discoverable except by temporarily paralyzing the ciliary muscle.

4. Relation of Pupillary Area to Axial Adjustment.

Pupillary change usually accompanies change of accommodation, though not an exact index of it. During the recent experiments measurements were made of the diameter of the pupil, the intensity of the light being kept as nearly constant as possible. When axial convergence takes place, the objects being as far as can be seen distinctly, the pupils first contract, then relax perceptibly, but do not recover their previous size even after binocular vision seems perfectly distinct. When axial divergence takes place, the objects being near, the pupils dilate noticeably, and then diminish very slightly. From the following table of measurements it is seen, on comparison with the curves of fig. 4, that for the near-point, when $\alpha = -5°$, the pupil is larger than for the far-point when $\alpha = +3° 30'$. If α exceed $45°$ there is no perceptible variation of the pupil when far-point and near-point are successively regarded.

Pupillary areas, in square millimeters.

Optic angle,	$-5°$	$+3° 30'$	$+25°$	$+60°$
Maximum area,	12·4	7·0	3·0	1·2
Minimum area,	8·2	4·2	1·7	1·2

For a given value of the optic angle it is quite probable that pupillary variation is proportional to change of ciliary contraction; but the table shows that it is much limited by association with variation of contraction in the rectus muscles. The dissociation between focal and axial adjustments is but rarely complete when the convergence is strong. If diffusion circles on the retina are very small, while the image is bright, we easily fail to notice them, and it is not easy to determine when perfect distinctness of vision passes into slight indistinctness. The range of perfect accommodation is doubtless much narrower even than is indicated in the curves of fig. 4, granting that the observer's eye is perfectly free from such defects as would produce permanent error of refraction. Since the size of diffusion circles depends upon the area of the pupillary opening as well as upon error of refraction, permanent or temporary, and since distinctness of focalization is indispensable to success in binocular vision, either with or without the stereoscope, the bearing of these experiments upon the theory of binocular perspective is obvious. Though many of them are straining, they are performed to some extent, more or less unconsciously, by every one who uses a stereoscope that is ill-adapted to his eyes and stereographs that are improperly mounted.

5. The Operation of the Will in Vision.

In reference to the operation of the will upon the muscles of the eyes, Helmholtz states that we are limited to efforts for the attainment of single and distinct vision. The French edition of his great work on Physiological Optics received the last corrections of the author, and from this the following quotations must be made:

"Il faut remarquer en général que, dans toutes les mouvements volontaires, notre volonté ne tend jamais qu'à atteindre un résultat extérieur nettement déterminé et perceptible par lui-même."—*Opt. Phys.*, p. 613.

"Il résulte de ces faits que la relation qui existe entre les mouvements des deux yeux n'est pas commandée par un mécanisme anatomique, mais qu'elle se modifie, au contraire, sous l'influence de notre volonté: la seule limite réside dans le fonctionnement de notre volonté que nous ne savons pas appliquer à un but autre que celui de voir les objects simples et nettement."—*Opt. Phys.*, p. 617.

The experiments just described show that the will may be directed to the attainment of other ends than single and distinct vision. Although not affected with any paralysis or other weakness of the muscles of the eyes, and hence never troubled with involuntary double vision, I find it quite possible at any time to converge or diverge the visual lines without using any external points of fixation, thus securing two separate monocular images of a given object, keeping these apparently separated at any desired angle within certain limits, and fixing the attention alternately upon either image separately, at pleasure. A door knob, for example, may be distinctly seen double by diverging the visual lines a few degrees, and either separate image may be thus examined. Discarding the use of one eye, this externally projected image may be varied at will, in sharpness of definition, apparent size, and apparent distance, by strong voluntary ciliary contraction. This power was of course never suspected until revealed by special experiment; but there is no reason to suppose that it is not possessed by most others whose eyes are healthy.

6. Binocular Vision and Binaural Audition.

The discoveries in reference to binaural audition, made during the last few years independently by Professor Silvanus Thompson,* of England, and Professor A. M. Mayer, of this country, are interesting, not only as additions to our knowledge of physiological acoustics, but also in relation to the phenomena of

* Philosophical Magazine, Oct., 1877, Nov., 1878, and Nov. 1881.

physiological perspective. The localization of sounds has been found to be much affected by the mode in which the waves are conveyed to the separate ears. The same tone may be perceived as if produced at the back of the head, or from the two sides, or from a point obliquely in front, while the position of the true external source is unchanged, the perception being involuntary while the conditions are adjusted at will. The judgment of distance by the ear is far more uncertain than by the eye, there being no other criterion than the degree of energy of the vibrations which give rise to sensation; but the perception of direction may be modified by imposing special conditions, such as fatiguing one ear with a given tone and then listening to the same with both ears. For a fixed position of the eye, the perception of direction may be modified at will by methods already described, or by pressing upon the eyeball, while that of distance is also subject to variable conditions. Although the binaural estimate of direction and distance may be made less uncertain by properly adjusting the position of the head to the wave front, or, as in the case of the lower animals, by directing the two ears at will toward the source of sound, no one has attempted to apply geometry to the binaural localization of sounds. Its application to binocular vision is now found to be wholly unreliable in the very department for which it was deemed most satisfactory, that of stereoscopic perspective.

7. Effect of Experience in Vision.

Assuming that focalization is distinct, the position of the external focus, conjugate to that on the retina, is calculable, but for distances exceeding a few metres the variation in thickness of the crystalline lens becomes so slight that as an element in forming judgments of distance it may be practically discarded. We are justified in saying therefore that there is generally no recognizable relation between the character of a retinal image and the distance of the object pictured, any more than between the composition of a sound image and the position of its external source, except so far as personal experience serves to establish it. Experience, both visual and auditory, begins in infancy, long before the power to analyze sensations is developed, and in putting an interpretation upon these, association is the chief determinant. Strong contraction in the ciliary and internal rectus muscles is unconsciously associated with nearness of the point of fixation, and this is pictured upon the yellow spot of the retina. No one whose eyes are healthy has any consciousness of possessing any retina except in relation to external objects, or any tympanum except in relation to

aerial disturbances, or any ciliary or rectus muscles except in relation to variation in position of the point on which attention is fixed through the medium of retinal sensation. We might expect therefore that unusual muscular conditions would cause misinterpretation regarding the whole field of view, but especially those parts which correspond to that portion of the retina which is most frequently impressed and most sensitive. Under strong muscular contraction, arousing abnormal vision, it is hence not remarkable that an object pictured on the central part of the retina should be misjudged as smaller and nearer than one of the same real size and distance pictured upon marginal portions; and that the illusion should disappear as soon as the muscles are relaxed and normal conditions thus restored.

There is one psychologic aspect of the present investigation of which I have not been unmindful, particularly in reference to the effects of extreme voluntary ciliary contraction. In recording the results of even normal vision every astronomer applies what experience has taught him to be his personal equation. In the present experiments in abnormal vision, some of which are not easy, the personal equation cannot well be ascertained. While there has been given what I believe to be a correct account of phenomena actually observed and not consciously anticipated by myself, there must remain, for a time, an element of uncertainty, how much may be due to personal peculiarity and how much to qualities belonging generally to the human eye. The most careful of observers may be occasionally mistaken in attributing to physical conditions what is really the subjective effect of unconscious expectant attention. The very consistency of these phenomena with the empirical theory of vision might cause a predisposition to certain perceptions that might not be so successfully attained by one who is thoroughly committed to the opposite school of thought.

8. Theories of Binocular Perspective and Relief.

The reasoning already expressed suggests additionally the futility of attempting to refer our binocular perceptions exclusively to any single condition such as the external intersection of visual lines or the internal recognition of double images in one part of the field of view while another part is scanned, applicable as these conditions may be when considered simply as elements that may at times enter into our interpretations. In their application to stereoscopy the physiological elements of perspective imply no contradiction to the laws of mathematical perspective which must be applied in constructing the stereo-

graph. The relative distances in the drawing being fixed, variation in muscular sensation may modify the imagined scale of measurement, while ratios are sensibly the same as before. The beautiful results attained by Professor W. B. Rogers* in determining the form of the binocular resultant are applicable to projecting lines that intersect, and not necessarily to visual lines, since the perception is attained when these are divergent. The same remark is true of Helmholtz' admirable mathematical discussion† of the stereoscope, in which he makes no provision for optic divergence, although elsewhere he refers to the possibility of stereoscopic vision by this method. It is but due him to state that, under the conditions assumed, he closes his discussion with the remark, "These conditions are not always fulfilled for the photographic proofs and the stereoscopes of commerce."

Associated muscular action, to which special prominence has been assigned in the present series of papers, is in like manner incapable of explaining all the phenomena of stereoscopy. Much stress has been laid upon it because it covers all the facts that have hitherto been referred to visual triangulation, and additionally those of optic parallelism and divergence, assuming that the stereoscopic displacement on the card is large enough to permit the perception of double images in the background when the foreground is viewed, and that the time of illumination is sufficient to allow free play of the eyes. The perception of stereoscopic relief is possible when the stereoscopic displacement is so minute that no sensible motion of the eyes is possible and no duplication of any points in either foreground or background can be perceived with even the keenest vision. The stereograph has been repeatedly examined, and relief distinctly perceived, successively with natural and reversed perspective, when illuminated by the electric spark, the duration of the retinal impression being thus reduced to a minute fraction of a second. Undoubtedly the play of the eyes often enables us to become at once sure of interpretations that would be enveloped in uncertainty until after several trials by momentary illumination. In ordinary vision we rarely ever notice the duplication of images on the hither and farther side of the point of fixation, partly because the attention is apt to be confined to the point fixed, but also because other points are not distinctly focalized upon the retinas, and the majority of these imperfect images fall on retinal parts that are not central and hence are deficient in sensitiveness.

It has been suggested‡ that the perception of relief is *by*

* This Journal, II, vol. xxi, pp. 91, 173, et seq.
† Optique Physiologique, p. 842, et seq.
‡ LeConte, Sight, p. 151; Appleton, 1881.

means of double images, and that the mind *instinctively* distinguishes between those made by objects that are respectively farther and nearer than the point fixed. This last proposition would be hard to demonstrate experimentally, but even when the attention is not specially given to the double images, these may, and probably do, play an important part as elements in the unconscious formation of judgments. Again it has been stated* that we see *at long and short distances at the same time*, because the retina has thickness and transparency, and images are focalized at different depths beneath its surface. Here again we can neither affirm nor deny in answer, although recognizing the fact that the crystalline lens, being one of short focal length, has in consequence of spherical aberration considerable "depth of focus." It is moreover fluorescent, irregular in structure, and imperfectly centred. Perfectly sharp focalization is hence impossible, as shown in the radiated appearance of stars and the irradiation about any brilliant surface like that of the crescent moon. These optical defects may be in some respects advantageous in ordinary vision. If Towler's theory be true, though not demonstrable, it may partly explain the possibility of binocular combination when the differences between the two pictures are so minute that the perception of double images in any part of the binocular field is impossible. Some idea can be formed of the minuteness of the stereoscopic displacement actually necessary when we consider that Mr. Warren De la Rue succeeded in obtaining a stereograph of the sun, from which by stereoscopic vision, the ridges of the faculæ could be perceived in sharp relief. On the stereograph of the moon, to which reference has been more than once made, the elevation of mountain ranges and solitary peaks, and even the inequalities of the supposed dead sea bottoms can be clearly seen. The crater Copernicus and the lunar Apennines stand forth particularly boldly, and the ridge that divides the bed of the heart-shaped "Sea of Serenity" can be easily traced. Any one who has undertaken the preparation of a stereograph with the pencil or pen knows how very difficult it is to avoid the production of roughness in the combined image at places where smoothness is desired. No two impressions from the same type can be taken that will not present some inequalities when stereoscopically examined, and no two groups of type representing the same sentence can be so accurately adjusted as not to betray imperfection when subjected to this searching test.

In what has been said of "corresponding retinal points," no mathematical meaning has been assigned to this expression, and obviously none can be. It is generally thought, but has not been experimentally demonstrated, that each rod or cone on

* Towler, The Silver Sunbeam, p. 310; E. & H. T. Anthony, 1879.

one retina has its mate in the other, and when such a pair are simultaneously impressed they convey but a single impression to the brain. If in each eye the retinal image is so small as to cover but a single rod or cone, we have thus the so-called *minimum visibile*, and by calculating the retinal area corresponding to the smallest object that can be separately seen at a given distance the diameter of such a retinal element has been estimated. There are strong reasons however for doubting the validity of this theory of corresponding retinal points. All we can affirm is that experience, acquired individually and probably with exceeding rapidity in consequence of inherited tendencies, has taught us to interpret retinal sensations that are slightly different in the two eyes, as the signs of an external object possessing three dimensions in space, when the images are made upon parts of the concave surfaces that bear to each other the mathematical relations imposed by the presence of such an object in normal binocular vision. Sensation is never confined to a point, or pair of points, but only to areas. If we grant the correspondence between small retinal areas, simply as the result of experience rather than as a truth in anatomy, it is not hard to understand how a pair of slightly dissimilar images may produce upon the brain a modified impression, even when no duplication in any part can be perceived. It may be quite possible for the *quality* of a retinal sensation to be modified while the additional impression, apart from that which it modifies, would be imperceptible. From what we positively know regarding sensation in the ear, we are justified in applying similar reasoning tentatively to sensation in the eye.

It is well known that no perfectly simple sound is produced by the voice, or indeed by any musical instrument; but that the difference in quality between two tones nominally the same from different sources is due to minute modifications upon sensations, corresponding to attendant waves, most of which are too faint to be perceived without instrumental aid. A well trained ear however is able to pick out some of these overtones, even when two different voices are singing nominally in unison. The brain of the listener receives two sound-images, each complex, that approximately coalesce. By special attention he singles out a few musical salient points and thus perceives double images in the background; but the great majority of the attendant modifying tones are so faint and so inextricably mixed together that it is impossible to do more than perceive their resultant effect upon the fundamental about which they are grouped. The rich combination of all stands out in strong musical relief, when compared with the impression from either voice alone, or with the sweet but thin sound of the tuning fork that sings forth the same fundamental pitch.

This comparison relates to the combination of sensations, whatever may be the cause of dissimilarity among the components of the group. In the case of audition the modifications imposed upon the sensation are due to combination among waves varying in length from a small fraction of an inch to many feet. The graphic representation by curves is an arbitrary but exceedingly convenient method of indicating the quality of a sensation from what we know of the physical character of its proximate cause, however complex this may be. Waves of light are far more minute than those of sound, and we have no evidence of sensible interference among them in the production of binocular vision. We have not the data from which a binocular image can be graphically expressed as a curve, and thus compared with one of the monocular components. But the facts that are forced upon our attention suggest kinship between the *modes of sensation* in the two cases. The group of light images in the one eye forms a sum total that is different from that in the other, and the sensations they arouse are simultaneously conveyed to the brain. Experience comes to our aid, and the modified resultant is instantly recognized, even though we may be unable to perceive separately the minute modifications which give character to the two main components that form it.

I doubt therefore the anatomical theory of corresponding retinal points, and regard that of partially correspondent retinal areas as a substitute more in accordance with observed facts. This correspondence moreover must be considered mèrely the effect of association resulting from oft repeated experience, an association that is very quickly established, although a newborn infant does not seem to possess the power of single vision at birth. Such retinal areas should be considered as not sharply defined, as is shown by the phenomena of irradiation. A point impressed becomes the center of a minute area of disturbance, just as on the skin a point pricked with a pin is a center from which pain is quickly extended to an area. A group of such retinal areas are of necessity partially coalescent. The absence of sharp definition on the retina is one condition on which stereoscopy becomes possible through the apparent blending of images that are really dissimilar. If the dissimilarity is but slight, clear relief is perceived without the production of any sensible duplication of images in any part of the binocular picture.

In the light of these facts it is seen that the explanation of stereoscopy with perfectly similar figures, as given in my last paper,* was incomplete. It was known to be so at the time it was given, but my intention was to present only a geo-

* This Journal, April, 1882, p. 297.

metric discussion and reserve the remainder for a new paper. In performing the experiment for the first time, confusion is generally experienced; after a few moments the form of the binocular resultant is clearly perceived, and in subsequent trials the perception is attained much more quickly, all parts of the image being sensibly equally distinct at the same moment, if the inclination of the cards be not great. It is only when this inclination is considerable that play of the eyes becomes necessary, and the associated exercise of the rectus muscles thus furnishes suggestions that are complementary to the modification of retinal impressions just discussed. There are further experiments in regard to this mode of stereoscopy by momentary illumination, which I hope to make and publish at some future time.

New York, 40 W. 40th St., April 3d, 1882.

ART. XXXVIII.—*On the Flood of the Connecticut River Valley from the Quarternary Glacier;* by J. D. DANA.

[Continued from page 202.]

THE facts, presented in the preceding part of this paper, on the dimensions and velocity of the flooded Connecticut appear to make it certain that, during the era of the great flood, the pitch of the Connecticut valley was very much less than it is now, and that a change equivalent to 1 foot a mile from the Sound to Springfield, and 2½ feet a mile to Haverhill (and an undetermined distance beyond)—but corresponding to some curving plane between the extreme points—would not be too great to meet the requirements.*

But I have not said that the land over the interior of New England was depressed to the amount stated, or to any amount. It has been left an open question whether the change was a change in the sea-level or in the land-level.

Leaving this point for discussion in another number of this Journal, I here take up the subject of the disappearance of the ice from the Connecticut valley.

5. *The Retreat of the Glacier.*

The *rate* of retreat or disappearance of the ice along the Connecticut valley and from the Connecticut River drainage-area had a relation to the amount of discharge of the flooded river: and the time required for the retreat a like relation to the amount of ice to melt. But there are various unknown

* This curving plane would make right the heights for Middletown and Springfield on page 199, where the former is, by the assumed rate of pitch, the higher.

quantities connected with this subject, and only some very general conclusions can be looked for.

(1.) *Drainage-area.*—The present drainage-area of the Connecticut, north of Massachusetts, covers about 8,500 square miles; between the northern Massachusetts line and Hartford, 2,100 square miles; and between Hartford and the Sound, 450 square miles; making in all 11,050 square miles. The Hartford limit is here made to extend south of Hartford on the west, so as to include the whole drainage-surface of the Farmington River, which joins the Connecticut north of Hartford. The area covered by the Quinnipiac River might also be added, since the overflows from the Connecticut River valley, at Northampton and south of this point, so far as they emptied into the Sound by a separate outlet, reached it by the Quinnipiac valley. But the area is not over 50 square miles.

(2.) *Amount of ice over the drainage-area.*—The mean thickness of the ice in and west of the White Mountain region, judging from the glacial scratches on the White Mountains, and on Mt. Mansfield of the Green Mountains, was probably about 5,000 feet; at the sources of the Connecticut River, 75 miles north, on the borders of Canada, at least 6,000 feet; and to the south near the Massachusetts border, not far from 3,500 feet. The mean thickness for the whole area, 8,500 square miles in extent, thence deduced, is about 4,500 feet; and, if so, the whole amount of ice covering the drainage-area north of Massachusetts, at any one time before melting had made much progress, would have been about 7,250 cubic miles.

Over the more southern part of the drainage-area, 2,550 square miles in extent, the mean thickness was probably near 2,000 feet. In this the thickness along the north shore of Long Island Sound is taken at 1,000 feet; and it is put at this figure rather than lower because the glacier continued flowing southward and eastward for at least 34 miles, it having extended across the Sound, and landed large bowlders, some of more than a hundred tons weight, with a large amount of moraine material (referred to the terminal moraine) on the south side and other parts of Long Island; and the chief part of the fall necessary for motion came from a northward increase in the thickness of the ice, the Sound being a shallow trough 20 miles or so wide. A slope of 0° 25′ would give a pitch of 38·39 feet per mile (= 1 : 137·5), and make the thickness along a line 34 miles north-northwest from the south side of Long Island, 1305 feet; and one of only 0° 20′ would give a pitch of 30·72 feet per mile (= 1 : 171·8), and a thickness of 1,044·5 feet.* On the above assumption the amount of ice in this part of the drain-

* In Greenland a thickness of more than 900 feet exists according to Helland, at the very end of the Jakobshavn glacier, this being proved by the thickness of the icebergs broken from it.

age-area at any given time would have been 965 cubic miles, making the total about 8,215 cubic miles.

(3.) *Sources of the Water.* — Melted ice and unfrozen waters from the precipitation over the drainage-area were the sources supplying the flooded streams.

The precipitation, to produce the snows needed for the Glacial era, must have been large, much larger than now. The amount at the present time for the part of the drainage-area north of Massachusetts averages 42 inches a year, and for the part south, 46 inches; while on Mt. Washington, just east of the area, the annual fall is between 55 and 80 inches. In 1873, 78·56 inches were registered on Mt. Washington.

In the Glacial era the conditions must have occasioned still more abundant precipitation. The ocean was, in all probability, much warmer than now, either through the shallowing of the Arctic seas, which shallowing would have diminished much the southward currents of cold polar water, and also the extreme northward flow of the Gulf Stream; or in some other way. In addition, the border region of the continent, in these and higher latitudes, had not merely snow-clad heights, but ice-made heights, from the sea-margin to the top of the lofty ice-range. Moreover, the region of New England was within the more seaward portion of the icy slopes.

The relations were consequently like those between the tropical Indian Ocean and the seaward mountain ridges of India, though less extreme. In India, at many localities, the mean annual rain-fall is 100 to 160 inches, while at three stations on the Ghats, in Bombay, it is over 250 inches, and at one in Assam, nearly 500 inches. In such facts we have seemingly sufficient reason for estimating the average annual rain-fall of the Connecticut valley during the Glacial era to have been at least as high as 120 inches. The present high average on the isolated White Mountains is good evidence that 90 inches would be too small an estimate, and the more so since during the four months in which half of the precipitation there takes place, June, July, August and September, the slopes are, with small exceptions, free from snow—Mr. Schott's tables (page 56) making the mean for the four years of observations 67·12 inches, and for the four months just mentioned, 33·83 inches.

The facts make it apparent, whether the above estimate be precisely right or not, that the amount of precipitation was far greater than in any modern glacial region. And the same must have been true for the southern portion of the ice-slope, *west* of New England, over which would have blown moisture-ladened winds from the region of the great Mediterranean sea of the continent; though possibly, owing to the distance from that sea and the position of the Appalachians, the annual precipitation

north of the Ohio may have been increased somewhat less than over New England.

From the condition of Greenland,*—a semi-continent, covered by a continuous glacier-mass more than a thousand miles long

* The chief sources of information with regard to the interior of Greenland are: (1) the exploration, in July, 1870, of Nordenskiöld over the ice for 28 miles, sixty miles south of Jakobshavn, near the parallel of 68° 20′, (*Geol. Mag.* for 1872); and (2) that of Lieutenant J. A. D. Jensen, in 1878, with Mr. A. Kornerup as geologist, who started on the Frederikshaab glacier, between the parallels of 62° and 63°, and traveled 47¼ miles in an E.N.E. direction (*Meddelelser om Grönland*, Copenhagen, 1879, Part first).

1.

The black part, ice; white, land; shaded, water; J. N., Jensen's Nunataks; D. N., Dalager's Nunataks; white lines on the black, crevasses; arrows, glacier-flow.

Nordenskiöld saw an undulating surface of ice, white and moraineless, much intersected by crevasses, and furrowed by innumerable rivers, and one "copious, deep and broad river between banks of blue ice," whose banks he followed until he saw "the whole immense mass of water rush down a perpendicular crevasse into the depths below." There was dust over portions of the surface, and with it, as detected by his companion, Mr. Berggren, great quantities of a minute alga, in the form of threads of usually 4 or 8 cells, along with some *Protococcus nivalis*, and, in one place, so much of the material lay together, (collected by streams then dried up), that it had become putrescent in the sun, "so as to emit a most unpleasant odor, like that of butyric acid." Nordenskiöld says that the algæ tend to make rapid melting of the ice under the summer's sun. The height reached by Nordenskiöld

and several hundred wide, and leaving outside only a fringe of fiords and islands generally but 30 to 60 miles in width,—we may derive many facts illustrating this subject. We learn from it that a continental glacier (1) would have crevasses of indefinite number and extent, either transverse to the direction of motion, longitudinal, or radial, according to the bottom over which it moved and the attendant conditions; that (2) it would be covered extensively, in the warmer season at least, with fresh-

was 2,200 feet, and the mean slope of the ice-surface on the way, as deduced from the height, was 0° 26′, equivalent to 40 feet per mile (1 : 132).

Lieut. Jensen, at the limit of his inland ice journey of 47½ miles from the foot of the Frederikshaab glacier, reached a cluster of rocky peaks, rising from beneath the glacier—*Nunataks* of the Greenlanders (J. N., fig. 1). The heights above the sea of the four largest were severally, commencing to the north, 5,623 (*g*), 5,184 (*i*), 5,654 (*k*), and 5,580 (*m*), feet. (See fig. 2 below.) From these peaks, which stand like islands in the sea of ice, moraines of stones and earth (some of the stones 20 feet in their dimensions) extend for 1 to 2½ miles, (*m′*, *m″*, *m‴*, *m⁗*, fig. 2) and dust, by the aid of the storm winds, is drifted off for wide distribution over the glacier. The moraines, after a short outside existence, disappeared beneath the ice, the stones dropping down the crevasses that were from time to time opening (the account says) as the glacier moved on. The varying direction of the moraines, and the eddies in the flowing ice, due to the obstructing ridge (of which the Nunataks are the peaks), which these directions indicate, are remarkably instructive. The arrows show the inferred direction of movement. The moraine *m′*, 2½ miles long, is made mostly of polished stones, which appear therefore to have traveled far, and not to be the debris of the nunataks. The *m″* and *m‴* have no connection with any visible nunatak. At *b* is a lake 824 feet in diameter, and toward it the ice around slopes from a height of 4,900 feet to that of 4,120 on its borders. The glacier had a height at *l*, east of the nunataks, of 5,150 feet. The slope of the ice-surface for the distance traversed averaged 0° 49′, or about 75 feet per mile (1 : 70); and it was evident that the movement of the glacier depended on this slope. Crevasses were numerous along the route transverse to the line of movement as well as longitudinal and radial (see fine lines along the route on fig. 1); and fresh-water streams were common, and water-falls also.

2.

The largest of Jensen's nunataks consisted mostly of hornblende schist in bold flexures, with mica schist and gneiss. Twenty-seven species of plants were collected from them by Mr. Kornerup, and among these were the grasses *Luzula hyperborea* and *Carex nardina*, widely spread, *Trisetum subspicatum* and *Poa trichopoda* in scattered tufts; the sorrel, *Oxyria digyna*; the white-flowering *Cerastium alpinum* and *Saxifraga oppositifolia*; the little blue-flowering *Campanula uniflora*; *Potentilla nivea*, *Ranunculus pygmæus*, *Silene acaulis*, *Cassiope hypnoides*, *Armeria Sibirica*; and the yellow-flowering poppy, *Papaver nudicaule*, which was taken

water streams, ending their short career by a leap down the crevasses; and that such streams might often attain large size before making their plunge to sub-glacial depths; that (3) the melting at surface and thereby the wasting of the ice would probably have been promoted by a wide distribution of microscopic algæ; that (4) sub-glacial streams would have been numerous and large, and that they might have kept up their flow even through the coldest season; that (5) sub-glacial streams, flowing out from beneath the discharging glacier, would have transported fine earth and clay, the results of glacier abrasion,

from the top of the highest nunatak. Of these, the *Luzula*, *Oxyria*, *Trisetum*, *Silene*, and *Cassiope* are among the Arctic species left on the White Mountains, while the *Saxifraga* is found on the Green Mountains (Asa Gray.)

The fringe of fiords and islands along Western Greenland, 30 to 60 miles wide, is mostly, as figured by Dr. Rink in his Danish Greenland, outside of the great continental glacier. The discharging ice descends into the heads of the fiords and thus makes the so-called glaciers; and here, under the action of the tides on the free end where not thick enough to lie on the bottom, great masses are broken off to drift away as icebergs. This border region, in contrast with the interior just east of it, rises into numerous peaks of 3,000 to 4,000 feet, and, about Umanak-fiord, in north Greenland, there are summits of 6,000 to 7,000 feet. It is mostly free of ice below a level of 2,000 feet and partly so above this limit; yet over its higher summits there are local glaciers. Figure 1 shows,—in the part left white,—the breadth of this fiord-border north of the Frederikshaab glacier, and, also, the interior ice descending into some of the fiords to varying distances, and failing to reach others. At the south end of Greenland, the interior ice is hardly 30 miles distant from Cape Farewell, although near 60° N.

To aid the reader in understanding the conditions in Greenland, the following climatal facts are here cited from Rink: The mean annual temperature at Lichtenau, in 60° 31′ N., is 33·2 F.; at Godthaab, in 64° 8′ N., 27·8 F.; at Jakobshavn, in 69° 14′ N., 22·6 F.; at Upernivik, in 72° 48′ N., 13·3 F.; at Jakobshavn, annual range of monthly means 0·3 F. to 45·3 F., and at Godthaab, 11·8 F. to 44·4 F.; temperature of the soil, 4 feet under ground, at Godthaab varies during the year between 31·5 F. (March), and 40·1 F. (September); the height above the sea of the snow line in Danish Greenland is about 2000 to 2200 feet.

In July, 1875, Helland, of Norway, made measurements on the glacier in the Jakobshavn fiord (*Om de isfyldte Fjord og de glaciale Dannelser i Nordgrönland*, and *Quart. Journ. Geol. Soc.*, 1877, p. 142). He determined the width to be 4,500 meters, and the slope of the ice-surface to be less than half a degree. Yet the rate of motion he found to be, at middle, 20 meters per day; 400 to 450 meters from the sides, 15 meters; and close by the side, 0·2 meters per day. This rate is 15 times greater than has been observed in the Alps and needs confirmation. The rate for the smaller glacier of the fiord of Torsukatak, in 69° 50′, he made half less. The rapid flow of the former he attributes to the pressure of the mass of the interior ice.

Helland states that in his views over the interior ice from five high peaks of the fiord border, between 69° 10′ N. and 71° 15′ N., he saw only ice, like a great sea, lying at a much lower level than these peaks but rising slowly inland and forming an undulating sky-line. The surface of the glaciers in the fiords was mostly free from stones, except at the margin. At the Jakobshavn fiord the discharge of ice in July was equal to one large iceberg a day of about 16,000,000 cubic meters. The discharge continues in the winter, as the icebergs set free prove, but at a slower rate. He observes also that the amount of glacier discharged as ice is far less than that which passes out as water beneath the glacier. The mean amount of mud discharged by the waters flowing from 6 glaciers he found to be in July and August, 1875, 727 grams in 1 cubic meter of water (or in 100,000 grams very nearly). According to the fishing lines of the Greenlanders, the depth of the Jakobshavn fiord is about 390 meters, or 1280 feet.

to make aqueous deposits, which might or might not be free from stones or bowlders.

Dr. Rink, the Greenland explorer, and for many years, Government Inspector of South Greenland, states, in his "Danish Greenland," (1877) that the sub-glacial streams which flow out from beneath the ice in the ice-discharging fiords, are very copious and sometimes violent torrents, making whirlpools a hundred feet in diameter as the waters escape from sub-glacial confinement; that they probably branch widely over the country like a regular river system; and he refers to the authority of the natives, as well as to the observations of explorers. He observes that, according to his estimate,* out of the 10 inches of annual precipitation—the mean amount—only 25 per cent, or 2½ inches, are needed to supply the loss of ice from the discharge of icebergs; and that the rest, apart from what is lost by evaporation, makes up the amount of loss from melting, indicated by the sub-glacial waters. An annual supply to this end of only 6 or 7 inches could not produce the largest of rivers.†

From these facts we may hence assume that the great glacier of Eastern North America would have had

(1) Crevasses, with only local exceptions;

(2) Surface streams flowing with nearly pure waters over nearly pure ice, suddenly becoming sub-glacial by descending the crevasses; and

(3) Sub-glacial streams as much more extensive than those of Greenland as the precipitation was more copious and the drainage-areas larger.

If in Greenland 60 or 70 per cent of the precipitation is a measure of the amount of water contributed to the sub-glacial streams, 60 per cent. may have well been the amount in New England during the progress of the Glacial era; and this, with an annual fall of 120 inches, would have been 72 inches or 60 inches with one of 100 inches. This estimate can hardly be over-large considering that tropical and warm-temperate regions were not far distant, and that the summers, if it were a fact that they were shorter than now, were proportionally hotter. When the glacier began its decline, the proportion of the precipitation becoming sub-glacial would have gradually increased, and so on to the end. And, consequently, as the amelioration of the climate made progress, the melting would have augmented in rate through the increasing warmth of the winds. The fact that glacial conditions tend to perpetuate themselves is well

* Based chiefly on the flow of ice in the Jakobshavn fiord, as measured by Helland.

† According to Helland, the mean precipitation at Jakobshavn for the year between July 1873 and July 1874, was only 8½ inches, and for the year following, 7 inches.

illustrated in the present state of Greenland. But in North America, situated on the wind-receiving side of a warm-temperate and tropical ocean, whose tropical waters lay against its southern border for more than a thousand miles, the strength of the tendency would have been severely tested.

Pressure and friction would have been other sources of melting; and perhaps, in view of the breadth of the area covered by the glacier, some small amount may have been occasioned by subterranean heat.

(4.) *Mean annual discharge of the Connecticut River at the time of maximum flood.*—The data for calculating the amount of discharge of the flooded river are: (1) the estimated mean width of the river, 2,500 feet, (the reduced estimate); (2) the mean depth, 140 feet; and (3) the mean velocity, which we may take at 3 and 4 miles an hour. The length of the river from Wells River to the Sound is 200 miles. Hence, 66⅔ hours for 3 miles an hour, and 50 for 4 miles, would have been required, to pass into the Sound the waters, that at any one time occupied the channel.

The estimate of 140 feet for the depth can hardly be too great, seeing that the highest terraces are, at the south, 200 feet high above modern low-water; and to the northward, 200 to 265 feet. The view that the range of upper terraces, including the so called delta-terraces, mark approximately the high-water level, is the common one with all that believe the terraces to be of fluvial origin; and I feel under obligation to add, inasmuch as the different idea about the "normal highest terraces" and the "delta terraces" of the Connecticut, opposed in the earlier part of this paper, is from one of the New Hampshire Geological Reports, that this accepted view is that sustained and taught by Professor C. H. Hitchcock, the head of the New Hampshire Survey.

The amount of water lying in the channel at any one moment would have been 2·511 cubic miles; and the daily discharge, if the velocity were 3 miles an hour, nearly 0·9 (0·904) cubic miles; or if 4 miles an hour, nearly 1·2 cubic miles (1·205). At the two rates of flow mentioned, the amount of water discharged during a year would have been about 330 and 440 cubic miles.

(5.) *Annual loss of ice from the melting.*—Now comes the question how much of this water came from the melting glacier-ice, and how much from the cotemporaneous precipitation; or how much did the amount of water exceed the amount of precipitation?

If all of the water came from the ice, the amount of ice melted—taking its specific gravity at 0·92—would have been,

at the two rates of flow in the river, about 359 and 478·5 cubic miles.*

The amount from the precipitation to be deducted does not diminish greatly these numbers, even if all of it went to increase the floods. With a mean annual rain-fall of 120 inches, or 10 feet, this amount for the part of the entire drainage-area north of Massachusetts, would be only 16·1 cubic miles: and for the whole drainage-area to the Sound, 21·8 cubic miles.

This amount, 21·8 cubic miles, is too large by the amount which would have been lost through evaporation, soil-absorption, chemical changes in and beneath the glacier making oxides and hydrates, and in other ways. An estimate of these losses may be derived from the modern Connecticut.

The annual discharge of the Connecticut, as determined by measurements made under the direction of General G. K. Warren, of the U. S. Engineer Corps, by General T. G. Ellis,† at Hartford, Ct., where low water in the stream is almost exactly mean-tide level, it being only ·045 feet below it, was as follows for the years mentioned:

Year.	Discharge in cubic feet.	Year.	Discharge in cubic feet.
1872	638,070,000,000	1875	570,649,000,000
1873	727,160,000,000	1876	706,291,000,000
1874	733,103,000,000	1877	526,261,000,000

The largest of these sums, that for 1874, corresponds nearly to 5 cubic miles (more exactly 4·98).

The amount of annual precipitation over the drainage-area *north of Hartford* to the sources of the Connecticut (allowing 42 inches as the mean for the part north of Massachusetts and 46 inches for the rest north of Hartford), averages nearly 7·17 cubic miles. Consequently the amount discharged at Hartford in 1874, was 69·5 per cent of the precipitation; so that the loss in all other ways was little over 30 per cent. In 1877, a year of minimum discharge, the amount was 50 per cent.

The proportion of 70 per cent more correctly represents the condition in the era of the glacier-melting than that of 50; for it is the proportion during a year of great Connecticut floods: and these floods are dependent, in nearly all cases, (1) on the large amount of snow and ice of the winter or spring season, and to a considerable extent (2) on the frozen state of the ground over the hill slopes and much of the country, favoring an easy slipping of the waters over the surface without absorption by the soil,—conditions eminently characteristic of the era of the

* The specific gravity is here taken too high because of crevasses and want of compactness of glacier-ice: but this error, for the calculation of which no specific data exists, is not here of importance. Helland found the specific gravity of the ice of a large iceberg only 0·886, this low rate being due to linear air bubbles which thoroughly permeated it.

† Annual Report of the Chief of Engineers for 1878, Appendix B 14.

Glacial flood. This point is further exhibited in the following table, in which the monthly discharge of the Connecticut at Hartford, for the years 1872 to 1877, is given from General Ellis's Reports; and also the rainfall (snow included) for the same years at Lunenburg, Vermont, two miles west of the Connecticut, in lat. 44° 28′ N., long. 71° 44′ W., at a height of 1124 feet above the sea; Hanover, New Hampshire,—on the east bank of the river, in lat. 43° 42′ N., long. 72° 17′ W., at a height of 530 feet above the sea; and at Amherst, Massachusetts, in lat. 42° 22′ N., long. 72° 34′ W., at a height of 267 feet above the sea; the first, from tables kept by Dr. H. Cutting of Lunenburg; the second, from the Dartmouth Observatory at Hanover (received from Prof. R. Fletcher); and the third, from the tables of Prof. E. S. Snell. The table shows that the floods are not the immediate effect of great rains, and this is plain from mere inspection.* So in the Glacial era, the loss of water by soil-absorption was at its minimum, by growing vegetation almost null, and by chemical processes of oxidation and hydration† very small, while ice to melt and melting and frozen ground to flow over were essentially indefinite in extent.

An allowance, therefore, for the loss from evaporation and other causes, of 20 to 25 per cent, during the glacier-melting, cannot be far from right. Taking the mean of these numbers, 22½ per cent, the amount for the whole precipitation, 21·8 cubic miles, becomes 16·9 cubic miles as the part contributed to the discharge of the flooded river; and this in the state of ice is 18·4 cubic miles. Consequently all the discharge but this portion would have been derived from the wasting glacier—which makes the waste per year (p. 367) 460·1 cubic miles if the velocity of the river were four miles an hour, and 340·6, if three miles, the former equivalent to about 1·26 cubic miles per day, and the latter to 0·933.

(6.) *Disappearance of the Ice.*—If the loss of ice by melting were 460·1 cubic miles per year, and this amount continued to be the loss uninterruptedly, 8,215 cubic miles of ice would have been carried off in about 18 years; and if the annual

* One marked exception occurred in August, 1856, when the waters of the river reached a height of 23·33 feet at Hartford, or but 25 per cent short of the highest on record (that of May, 1854, when the height was 29·83 feet). But the precipitation at that time was enormous, having been in that August 12·132 inches at Amherst (Snell), and 11·68 inches at Wallingford, Connecticut (B. F. Harrison, in this Journ., xxi, 497, 1881).

† The loss by hydration here alluded to is in part that used in making the clay of the glacier discharge which is abundant in Greenland as well as in the products—bowlder-clay—of the Glacial era. Such clay has been made by the decomposition of the feldspar which the glacier-movements grind up, and when pure contains 13 per cent or so of water. The decomposition is a remarkable fact; but it is possible that the decomposition of the algæ of the glacier may have made carbonic acid to promote it.

	Discharge in millions of cubic feet.	Monthly precipitation.				Discharge in millions of cubic feet.	Monthly precipitation.		
		Lunenburg.	Hanover.	Amherst.			Lunenburg.	Hanover.	Amherst.
1872.					1873.				
Jan.	29,288	1·95	2·88	1·51	Jan.	64,430	3·85	3·05	3·01
Feb.	18,849	3·05	2·80	1·89	Feb.	45,123	3·35	1·75	3·17
March	22,437	2·70	3·24	2·87	March	43,316	4·50	3·54	3·18
April	130,966	2·07	0·40	2·20	April	181,780	2·65	1·32	1·74
May	82,164	8·20	3·42	3·11	May	138,720	2·64	1·21	3·91
June	63,696	7·34	4·34	3·25	June	26,400	2·00	1·74	1·59
July	26,918	7·25	5·64	7·07	July	18,982	3·95	5·91	2·93
Aug.	59,996	12·79	7·35	5·28	Aug.	17,270	2·50	1·52	3·47
Sept.	53,388	3·48	2·62	6·20	Sept.	17,718	4·75	3·83	4·77
Oct.	41,923	2·47	1·57	3·64	Oct.	66,780	5·45	5·57	6·36
Nov.	62,897	5·05	0·75	4·48	Nov.	39,915	2·22	2·38	3·51
Dec.	45,548	5·00	2·27	2·69	Dec.	66,726	2·65	1·65	3·31
Total	638,070	61·35	37·28	44·19	Total'	727,160	40·51	33·47	40·95
1874.					1875.				
Jan.	135,491	3·70	2·90	5·46	Jan.	17,079	2·60	3·92	2·90
Feb.	96,674	1·80	1·81	2·18	Feb.	27,023	4·92	1·20	3·62
March	71,721	2·25	0·68	1·35	March	49,032	3·05	2·55	4·20
April	62,031	4·05	3·40	6·03	April	146,049	2·85	1·92	3·33
May	139,213	2·95	3·26	5·22	May	111,437	3·73	3·94	2·19
June	74,110	7·06	3·44	5·06	June	35,607	5·70	4·98	2·89
July	55,018	4·98	5·07	1·16	July	22,991	2·55	2·70	8·15
Aug.	28,293	4·38	2·01	2·69	Aug.	31,971	3·40	4·07	6·16
Sept.	17,412	1·35	3·92	1·82	Sept	17,468	4·35	1·94	4·65
Oct.	19,310	1·15	1·30	0·85	Oct.	27,387	5·26	3·94	3·89
Nov.	15,865	2·71	1·92	3·54	Nov.	41,022	2·92	1·63	3·97
Dec.	17,965	3·07	1·08	1·17	Dec.	43,583	1·40	0·78	1·03
Total	733,103	39·45	30·78	36·53	Total	570,649	43·73	33·57	46·98
1876.					1877.				
Jan.	79,956	3·55	1·72	2·30	Jan.	17,500	2·15	2·70	2·52
Feb.	64,400	3·60	3·59	5·53	Feb.	18,491	0·65	0·00	0·36
March	93,866	3·02	3·50	7·10	March	93,253	6·40	2·57	6·97
April	160,756	2·75	0·52	3·10	April	110,247	2·35	1·97	2·45
May	155,521	4·70	1·32	3·96	May	45,374	1·05	1·03	1·93
June	41,008	7·05	3·46	3·87	June	20,907	3·00	4·07	4·59
July	22,016	5·25	0·00	4·84	July	25,475	4·22	8·48	6·47
Aug.	16,674	1·25	0·42	0·27	Aug.	22,946	5·95	4·01	2·79
Sept.	17,186	5·90	4·58	3·71	Sept.	18,089	2·05	0·91	0·91
Oct.	16,930	1·60	0·53	1·12	Oct.	31,772	4·70	4·67	6·98
Nov.	20,822	1·67	2·13	2·49	Nov.	75,825	3·65	3·81	5·44
Dec.	17,156	2·81	4·30	3·22	Dec.	46,382	1·87	0·96	1·02
Total	706,291	43·15	26·07	41·51	Total	526,261	38·04	35·18	42·43

At Norwich, on the west side of the Connecticut, opposite Hanover, the precipitation in 1872 was 41 inches, in 1873, 36·57, in 1874, 35·32; greater, in each case, than at Hanover. Schott makes the annual *mean* at Hanover 40·66.

The number of inches of snow in the above years at Lunenburg and Hanover was as follows:

Year.	Lunenburg.	Hanover.	Year.	Lunenburg.	Hanover.
1872	139·50	28·75	1875	122·45	106·73
1873	136·20	100·00	1876	90·85	113·00
1874	118·85	77·70	1877	42·75	32·00

loss were 340·6 cubic miles, the same work would have taken about 24 years.

8,215 cubic miles of ice are equal to the part of the glacier estimated to have occupied the drainage-area of the Connecticut at the time of maximum ice.

Supposing the above numbers approximately correct, we are still far from a knowledge of the time taken during highest flood for the disappearance of the ice from the valley. For in order to reach a conclusion it is necessary to know *also*,

(1.) The approximate amount of ice that was still in the valley when the epoch of maximum flood was reached, the estimate made being the amount at the time of maximum ice.

(2.) Whether the southward movement of the great northern glacier still continued, and was adding to the amount of ice.

(3.) What was the amount of ice contributed at that time by local glaciers within, or on the borders of, the drainage-area, which glaciers used some of the precipitation to make ice for melting.

(4.) Whether the melting, instead of continuing on uniformly, had its years or periods of interruption.

(5.) Whether the time of maximum flood was not quickly passed, even in one, two or three years, so that the discharge of the river was at its highest stage only for a very short time and rapidly declined to some limit, lessening thereby, both the rate of flow and the amount of water.

With doubts on points so important, this time-question must be set down as of impossible solution.

But while a definite answer is not to be expected, we may see reason for the following general inferences.

a. At the time of maximum flood the ice was not lying along the center of the valley producing the river by its gradual melting, and retreating northward as the river elongated in that direction.

The amount of water flowing off with a velocity of three or four or more miles an hour, making the great flood, was too vast to have been generated from a retreating body of ice in the valley.

b. If, as Greenland facts authorize us to believe, sub-glacial rivers of large size and energy were a universal feature of the Glacial era, these streams must have entered on a career of real progress when melting began in earnest. As they enlarged, the icy tunnels they had hitherto occupied would have become widened, and the sub-glacial chambers have extended themselves in all directions, undermining the heavy glacier. And as rapidly as this removal below went on, the deposition of the materials of the ground moraine—the stones, gravel, earth and clay—long before initiated—would have gone forward, covering

with till the glacier-buried land. But, subsequently, when the rising streams had volume enough to make the lower range of terraces, along the valleys, the roofs of the tunnels were probably, for the most part, gone. The ice still lay over the land, covering deeply the hills and mountains, but the wide channelways were open to the day. Evidence of this is afforded by the fact that these lower terraces, like the higher, are free, with rare exceptions, from deposits or droppings of till or of bowlders, such as would have come from an overhanging glacier. But outside of the terrace plains, up the hill slopes wherever the ice still remained in force, the till may have continued to fall, adding later to earlier till. Other evidence that the streams, as the floods rose and terracing went forward, were free from ice overhead is afforded by the Lake Champlain and St. Lawrence valleys. The shell-bearing sea-beach deposits at Montreal and on Lake Champlain could hardly have been formed from a thriving growth of mollusks and other marine species while the glacier lay over the whole region to the Gulf, a distance from Montreal of 500 miles. Whales (*Beluga Vermontana*, resembling much *B. leucas* Gray), and seals, inhabitants of the bays of Arctic coasts, would never have made their way beneath such a cover of ice to Lake Champlain, a region also under the ice. If then the St. Lawrence valley for more than 500 miles and the Lake Champlain region were free of ice, this may also have been true of the larger valleys of New England and the west.

c. It follows from the existence of a cut across the glacier along the St. Lawrence valley, from Lake Ontario to the Ocean, over 500 miles from east to west, and north of all New England, that at the time of highest flood New England was not receiving additions to its ice through the southward movement of the great northern glacier.

d. The Connecticut valley, at the time of maximum flood, derived its waters—apart from the portion supplied by precipitation—from the ice of the bounding slopes of the valley and of the wide tributary region. Consequently, the melting surfaces up and down the borders of the valley and over the slopes east and west to the limits of the tributaries would have been of almost indefinite extent, and the ice thus melted might have reached the amount deduced above—a cubic mile per day.*

* It will be observed that such conditions were well fitted, especially toward the mountains or in the region of the upper sub-glacial portions of the tributary streams, to produce and set afloat masses of ice laden with till, to float down a tributary to the Connecticut—a kind of work that must have begun early in the era of melting. Reaching the wide borders or flood-grounds of this river, they would, for the most part, have been pretty sure to have become stranded above and below the mouth of the tributary, owing to eddies and shallows; and there, as they melted, would have dropped their stones and earth, and thus have helped to make the coarse gravel and cobble-stone beds which, in

e. The length of time during which the waters stood at maximum height was probably less than five years. This accords with the following facts: that the terraces marking this upper level have much less extent than those of lower levels: that they are confined more narrowly to the vicinity of the mouths of the tributaries—the transporters and distributers of terrace-material; and that they are absent altogether from some long portions of the valley, except quite close to or within the mouths of tributary streams. As already remarked, the height of the flood may have exceeded, for a short time, the deduced heights by 10 or 20 feet: and a wide range of evidence of this may yet be made out.

f. The approximate length of the era occupied by the rise and fall of the flood is not a question within the range of this paper, and does not appear to admit of determination by any of the facts considered.

ART. XXXIX.—*On Brazilian specimens of Martite;* by ORVILLE A. DERBY.

THE opinion has lately been advanced (M. Gorceix, Comptes Rendus de l'Acad. des Sci., No. 7, 1880; Annals da Escola de Minas de Ouro Breto, No. 1), that the octahedral crystals of oligiste, so common in the metamorphic schists of Minas Geraes and known by the name of martite, are due to the transformation of pyrites. That this is true of some of the crystals, is beyond a doubt, but a recent examination of fine collections from typical localities indicates that a large portion should rather be considered as produced by the alteration of magnetite.

In a lot of 308 crystals ranging from 2 to 10^{mm} in diameter, collected by myself from a single locality near the village of Itambé, from a partially decomposed quartzose micaceous schist, 116 were attracted and for the most part freely lifted between the poles of a small horse-shoe magnet, and of these 57 were freely attracted by a much stronger bar magnet, only a few being lifted freely. 192 were not attracted by the horse-shoe magnet, and 251 showed the same indifference to the bar magnet.

many places to the north, compose the topmost portion of the higher terraces (indexes of the so-called "Kames.") The stones of such ice-floes would have been largely from *northern* sources, whatever the direction of the stream contributing them, as explained on page 89.

The liability of floating masses to strand, and to drift up stream as well as down, in consequence of eddies along the sides of a river, is illustrated in an instructive way on the Connecticut, in early summer, by the great crowd (sometimes millions together), of logs (tree-trunks) that float down it to lumber-yards in Massachusetts and farther south. Were it not for a large party of hands to look out for the logs and keep them moving, they would be generally ashore before going far on their course. The bends in the stream add much to the difficulties.

A specimen which was lifted freely by both magnets gave a black strongly magnetic powder, reacting with both ferri- and ferro-cyanite as well as with sulpheyanite of potassium. Another specimen not attracted by either magnet, gave a red powder which was feebly attracted between the poles of the horse-shoe but was not influenced by the bar magnet. This gave a characteristic reaction with sulph- and ferro-cyanite of potassium, but with the ferricyanite only gave an almost imperceptible blue precipitate which was immediately lost in the coloration due to the great excess of peroxide.

A specimen extracted from a greenish micaceous schist from Inficionado (locality from which martite was first described by Spix and Martius) apparently identical with the Itambé rock, but not decomposed, was strongly attracted by both magnets and gave a black strongly magnetic powder reacting for both per- and protoxide of iron. From a similar rock from Serro, partially decomposed, two specimens were extracted, one with a brilliant surface, the other somewhat dulled. Both were magnetic but in different degrees and both gave the reactions for the two oxides, but the powder of the first was black and of the second red. In another specimen from near Serro, small splendent, strongly magnetic crystals, giving a black powder, were embedded in compact hematite. In another small lot of brilliant crystals, all were so strongly magnetic as to form themselves in strings of four or five on both magnets.

In these experiments all possible gradations as regards magnetism and composition between typical magnetite and oligiste, were found. Specimens from the surface, or from much decomposed rock, will naturally be of pure oligiste, but present no external evidence of the change from magnetite unless it be an almost imperceptible dulling of the surface and a slight coppery luster.

ART. XL.—*A method for determining the flexure of a Telescope Tube for all positions of the Instrument;* by J. M. SCHAEBERLE.

IN all observations with the meridian circle only the maximum *astronomical* flexure of the telescope tube is determined directly from observations, two horizontal collimators being used for this purpose. This maximum flexure is then assumed to vary as the cosine of the altitude. I propose in this paper to give a very simple method by means of which the accuracy of this law can be tested directly for all positions of the tube, nothing but a small plane mirror, silvered by Draper's process, being required for this purpose. To this mirror is cemented a circular ring of cardboard of just sufficient thickness to prevent

the mirror from touching the object-glass of the telescope to which it is cemented with a solution of gum arabic. The centering is quickly done by shifting the mirror upon the spherical surface of the lens until the reflected image of the horizontal wire is nearly in coincidence with the wire itself.

In experimenting I have used a piece of plate glass; to test the surface of the same before silvering, the image of a distant brick wall, reflected obliquely from the glass surface, was examined with the aid of an excellent 2½ inch achromatic; from a number of pieces one was selected which gave a sharp image of both the vertical and horizontal joints for the same focus. A beautifully sharp and fine image of the wires will be given by such a mirror (silvered on the side toward the object-glass) although less than two inches in diameter.*

Let m_1 be the reading when the micrometer wire is in coincidence with the reflected image of the fixed horizontal wire, the telescope being pointed to the zenith, and let m be the reading for any other pointing. Let x and y be respectively the deflections of the object- and eye-ends of the tube from the positions these ends would have if there were no flexure.

L= focal length of objective.

l= distance of the object-glass from the line about which the flexure causes it to revolve. Placing the sines and tangents of very small angles equal to their arcs, the linear distance of the fixed wire from its reflected image, can be expressed by a very simple equation. For we have,†

$$\frac{2}{\sin 1''}\left(\frac{x}{l}-\frac{x-y}{\mathrm{L}}\right)=\frac{m-m_1}{\mathrm{L}\sin 1''}, \quad \text{therefore}$$

$$x\left(\frac{\mathrm{L}}{l}-1\right)+y=\frac{m-m_1}{2}.$$

Letting $\left(\frac{\mathrm{L}}{l}-1\right)=c$ and $\frac{m-m_1}{2}=\mathrm{F}$, we can write

$$\begin{aligned} cx &+y &&=\mathrm{F}\\ cx' &+y' &&=\mathrm{F}'\\ cx'' &+y'' &&=\mathrm{F}'',\\ &\text{etc., etc.} \end{aligned}$$

If f_0 represents the maximum *astronomical* flexure determined by means of two horizontal collimators, we have

* The focal length of the objective used is eight feet, and the wires made visible in precisely the same way as in taking nadir observations.

† The flexure really gives the tube a curved outline. In assuming that the change in the inclination of the mirror (due to flexure) is equal to $\frac{x}{l\sin 1''}$ (where l can usually be taken equal to the distance of the objective from the nearest face of the cube, to which the tube is bolted) only very small terms are omitted which can generally be neglected as inappreciable.

$$f_0 = x_0 - y_0$$
$$F_0 = cx_0 + y_0$$

hence we obtain

$$x_0 = \frac{f_0 + F_0}{c+1} \qquad y_0 = \frac{F_0 - cf_0}{c+1}.$$

Representing the zenith distance of the pointing by z we should have

$$(cx_0 + y_0) \sin z = cx + y = F$$
$$(cx_0 + y_0) \sin z' = cx' + y' = F',$$
etc., etc.

provided the law, that the flexure varies as the cosine of the altitude, is correct.

If the measured values F, F′, etc., are not satisfied by the above equations, the residuals will furnish data for obtaining the form of the function which, when substituted in the place of *sin z*, will best represent the observed readings. Any sudden *apparent* change in the flexure, as for instance the shifting of the object-glass in its cell, could be observed directly by keeping the eye at the instrument while it is being revolved. In some cases it might be well to fasten the mirror to the cell instead of to the objective. After the flexure has been determined, a thin strip of metal, the weight of which is exactly equal to the weight of the mirror, can be wound around the object end of the tube.

ART. XLI.—*On the dykes of Micaceous Diabase penetrating the bed of Zinc Ore at Franklin Furnace, Sussex County, New Jersey;* by BEN. K. EMERSON.

THE great open working at the Taylor mine in the "Buckwheat Field," at Franklin Furnace, is carried forward for a long distance with a width of perhaps fifty feet and a depth of ninety feet. As the limestone bed dips 90°, the walls are vertical, and as the ore body in this limestone bed has a steep pitch to the north, the mine is at its north end no longer worked out today, and as if planned specially for the convenience of the miners, a great vertical dyke 20–22 feet in width cuts the limestone and the ore bed at this point, and standing vertically stretches at right angles across the mine like the partition wall of a burned building. Seventy feet above the floor of the mine and again at the bottom passages have been cut through the dyke to get at the ore behind. From a point near the southern foot of the dyke, smaller dykes run up to the surface dipping 50° S., widening and narrowing and sending off small branches. These smaller dykes do not exceed two feet in thickness. Twenty feet farther south another dyke more inter-

rupted, and never more than six inches thick, runs nearly to the surface. This is the appearance of the dykes in the west wall. In the east, the dyke last mentioned is thicker and continuous from top to bottom, and the two small dykes which start on the west from the foot of the main one are widely separated. The effect of these dykes upon the zinc ores is remarkable. Large portions of the eruptive rock have exactly the appearance of an amygdaloid, but the amygdules are spheres of green willemite melted out of the vein, and such drops, together with franklinite could be found, though more rarely, in the coarsest rock, from the center of the main dyke. The smaller branches running down to a width of a quarter of an inch, and penetrating the richest portion of the ore—the "green silicate"—are changed to a depth of a quarter of an inch or more into a red jaspery mass, which retains its thickness as the dyke thins, so that dykes less than an inch in thickness are made up wholly of the brick red aphanitic material. In the thicker veins from which these filaments spring, the rock as a whole grows aphanitic toward the contact, while the scales of mica are even larger and are arranged parallel to the contact plane. The rock has the appearance of a very micaceous diorite; is brownish gray in the coarser varieties to nearly black in the finer, and varies from aphanitic to a coarser kind where the red mica is abundantly visible to the eye, and more rarely small black augites appear. It is of high specific gravity and effervesces abundantly with acid.

Microscopically it resembles closely the finer grained, dark colored elæolite rock from Beemersville which there seems to be an offshoot from the great main dyke, and it was for this reason as well as to observe the effect of the large admixture of the zinc silicate, investigated with the microscope. The resemblance was not sufficiently close to make it probable that the dykes of the two localities are of the same age and origin.

Under the microscope the foreign constituents, franklinite, zincite, willemite and calcite, are present in large quantity; the zincite easily mistaken for franklinite from its opacity but showing on the thinnest edges a deep blood red. The willemite is especially abundant, both in round drops apparently melted, —green, semi-transparent, fissured by many cracks, and showing feeble polarization,—and in broken crystals and large cleavage pieces. It resembles olivine in many ways, having a strongly marked but irregular rectangular cleavage and a very rough surface in thin sections, but it polarizes feebly and without bright colors. In this respect it differs from the normal willemite, which was sliced for comparison, from an inch outside of one of the small dykes, which resembles and rivals olivine in the brilliancy of its colors in polarized light.

The normal constituents of the rock are labradorite, augite, biotite, apatite, and probably magnetite, though one cannot well distinguish the latter ingredient from the franklinite. The colorless background in which the other constituents appear polarizes in large irregular patches, and for the most part shows neither cleavage nor any other characteristic by which it can be determined in its present decomposed state. A small portion, and this was increased by treatment of the slide with acid, shows multiple twinning with extinction at 10° on either side of the twinning plane, which would indicate labradorite, and it often combines with this a second twinning at right angles to the first. Another portion shot through with brightly polarizing scales of muscovite resembles a decomposing elæolite. Experiments with regard to gelatinization gave uncertain results, from the presence of so much zinc silicate, but some portions which did not seem to be willemite were removed by acid.

The abundant scales of biotite are with great frequency pierced by unusually large regular prisms of apatite, and decomposition has attacked some crystals at the edge, and following in on certain layers has changed them into an aggregate of colorless scales apparently of silica. The augite is of very light brown color, and appears in unusually long flat blades from the great development of the orthopinacoid. It is without absorption and extinguishes the light at 37° to the vertical axis. One finds in addition scattered crystals of much larger size (·2 mm.) which have the stout form and the color of common volcanic augite, and in several instances enclose large fragments of willemite and franklinite, or willemite and pyrite, and are also surrounded by a greenish zone of equable width which shows aggregate polarization, and consists of willemite and calcite.

In other varieties of finer grain, the elongated crystals of augite, and plates of red mica similar in shape and size to these, are more abundant and fill the field in reticulate arrangement, as do the feldspar needles in a common diabase. Moreover a gray or greenish gray semi-opaque substance, made up of scales and irregularly rounded particles, is gathered in great quantity around these crystals so that they are often almost wholly concealed thereby, and look like caddis worms in their cases. This is the seat of very abundant effervescence when the slide is treated with acid, but is only partly removed.

The feldspathic constituent appears here also as a colorless background, but more sparingly, and it is even more decomposed than in the coarser rock. It differs from this only in enclosing in abundance straight acicular microlites which are abundant also in the elongate augite crystals.

The finest grained black rock three inches from the contact

in the main dyke showed under the microscope a uniform gray ground difficult of resolution and resembling that of a phonolite, and regularly mottled by abundant small rounded opaque spots.

When examined with a power of 600, it is found to show small portions of a colorless isotropic glass which is crowded with minute particles apparently spherical scattered either singly or joined three or more together into moniliform rods, or, finally, densely agglomerated into the opaque spheres with which the ground is mottled. These products of the devitrification of the glass-base bear close resemblance respectively to the globulites, margarites and cumulites of Vogelsang, except that they do certainly polarize light.

On this background appear (*a*) large, often well-formed crystals of franklinite or magnetite, (*b*) very abundant clear hexagonal cross-sections which appear to be apatite, and others larger and clouded with the included microlites which may be nephelite, (*c*) small very regular sections of pale brown augite sometimes beautifully twinned, and (*d*) rare scales and needles of biotite. The augite crystals are often in flat blades, from the large development of the orthopinacoid, and extended in the direction of the vertical axis into long needles with well developed basal cleavage, which resemble somewhat needles of fibrolite, but extinguish the light at an angle of 37° with the vertical axis.

The larger crystalline constituents described are here scattered at a distance from each other in the semi-opaque ground mass and show a very distinct fluidal structure.

Finally, in the jasper-like material of the narrow red filaments, the ground is in thin section deep red and the material resembles the basic volcanic glasses in the difficulty of getting a transparent slide. The ground has however undergone complete or almost complete devitrification and the opacity is caused by the close aggregation of minute grains resembling those of the preceding variety but redder and even more minute than they, which gave when the light could be driven through the thinnest portion of the slide bright aggregate polarization.

Quite large pieces of included willemite and zincite occur abundantly. The zincite is of so deep color that by ordinary light most of the pieces are quite opaque and only here and there a blood red light penetrates a small portion of the surface. Curiously with crossed Nicols these masses of zincite glow out like carbuncles and the bright red of the background quite pales beside it. It is at the same time quite apolar.

A multitude of sharp hexagonal apatite cross-sections let the light through the dark ground, and the same pale brown augite and red mica appear as in the other varieties.

ART. XLII.—*On the Occurrence of Smaltite in Colorado;* by MALVERN W. ILES.

A vein of cobalt-bearing minerals, containing smaltite in predominant quantities, has recently been discovered near Gothic, Gunnison County, Colorado.

The vein is strong and well defined, indeed projects from a granitic formation in several places. This property, owned by the Sterling Mining Co., has as yet received but little development, but sufficient to lead one to believe it will be a commercial source for cobalt.

The vein matter contains much crystallized calcite, in which are irregularly distributed smaltite and erythrite. There is occasionally noticed a small amount of iron pyrites, and also very beautiful spongy leaflets of native silver. The vein is particularly free from siliceous matter, and up to the time of writing, we have been unable to detect any associated nickel minerals.

A sample of smaltite obtained from the surface croppings yielded the following results:

Co	11·59
Fe	11·99
As	63·82
SiO_2	2·60
Pb	2·05
S	1·55
Bi	1·13
Cu	0·16
Ni	trace
Ag	"
	98·89

A purer sample yielded 15 per cent. cobalt.

The Gem, and other mines, near Silver Cliff, Colorado, contain a number of nickeliferous minerals, and a small amount of cobalt.

The writer has recently detected a small amount of nickel and a trace of cobalt in an iron pyrites near Granite, Colorado; the latter mineral bears a striking resemblance to the nickeliferous iron pyrites found at Anthony's Nose on the Hudson River.

Chemist, Grant Smelting Co., Leadville, Colorado.

ART. XLIII.—*On the occurrence of Vanadium in the Leadville Ores;* by MALVERN W. ILES.

AT the Evening Star and Ætna Mines, I have detected a vanadium mineral, occurring in a pocket surrounded by a siliceous gangue, and also in a well-defined seam. In the Evening Star mine, the vanadium compound is associated with the so-called "hard carbonate of lead," and is found as an incrustation upon a dark chocolate-colored siliceous gangue, which gangue is in turn deposited upon nearly pure white quartz. The mineral in question has various shades of color, from red to yellow, and may be described as orange-red, reddish-yellow and lemon-yellow. Both the red and yellow specimens give a deep green solution when treated with hydrochloric acid, and not unfrequently the chocolate-colored mineral gives a marked amount of vanadic acid.

Prof. Silliman has called attention to the fact that a chocolate-colored mineral, found associated with vanadinite, also yields a perceptible amount of vanadium.* The reagents, upon which most reliance was placed to detect the presence of vanadium in the numerous specimens at my disposal, were hydrogen peroxide and potassium ferrocyanide; and the property vanadium compounds have of giving a green solution with hydrochloric acid was made use of. Portions of the brilliant colored incrustation were detached from several specimens, and submitted to a chemical examination; the results make it probable that the mineral is dechenite. The analysis is as follows:

SiO_2	36·86
PbO	38·51
ZnO	9·07
V_2O_5	9·14
Fe_2O_3	2·59
H_2O	2·41
CO_2	0·48
	99·06

In the estimation of vanadic oxide we used the method of precipitating the vanadium as a basic lead vanadate, and subsequently decomposing this salt by means of dilute sulphuric acid; the concentrated filtrate, after using the well-known precautions to ensure removal of all traces of lead, was transferred to a platinum crucible, the sulphuric acid cautiously expelled, and then strong heat was applied with a blast lamp until complete fusion of the vanadic oxide.

* This Journal, III. xxii. 200, 1881; Engineering and Mining Journal, Sept. 3d 1881; Transactions American Institute of Mining Engineers.

ART. XLIV. — *On Certain conditions attending the Geological Descent of some North American types of Fresh-water gill-bearing Mollusks;* by C. A. WHITE.

[Extracted in advance from the Annual Report of the U. S. Geological Survey for 1882, by permission of the Director.]

THE following remarks are extracted from an article entitled a Review of the non-marine Fossil Mollusca of North America. They are intended to apply especially to the fresh-water Mollusca of the Laramie and Eocene deposits of Western North America, and their descendants now living in the Mississippi drainage-system, and are offered as an explanation of the manner in which that system became stocked with its present characteristic molluscan fauna, and doubtless to a large extent, with its ichthyic fauna also.

When we attempt to trace the probable lines of succession or descent of the various types of Mollusca that now exist, the difficulty seems especially great, when casually considered, in the case of the fresh-water gill-bearing Mollusca.

The prevalence of the sea has always been practically universal; and the various movements which the earth's crust has undergone since life began in the sea, while they have repeatedly disturbed or destroyed the habitats of its molluscan denizens in certain localities, and many of the lines of genetic succession of types that had from time to time become established have been broken, there has evidently never been anything like such a general destruction of life in the sea as would either break or materially interfere with the greater part of the principal lines of such succession. In short, the marine field for the development and perpetuity of molluscan life has been ample and unbroken from the beginning to the present time, and we are at no loss to understand how continuous lines of genetic succession of its denizens may have extended down through all the geological ages, modified it is true, by immediately environing and cosmical causes, but still unbroken. We may at least conclude that if every molluscan species that now exists in the sea has not been lineally derived from the earliest molluscan forms that have existed in it, there have been no such changes of its physical conditions as would preclude such a possibility.

When we come to the study of the fossil pulmonate Mollusca, especially the land-shells, we have also little or no difficulty in understanding how it has been possible for continuous lines of existence of these mollusks to be preserved through successive geologcial periods upon any continental area, such for example as North America, notwithstanding the numerous and

great physical changes that have taken place within its area during those periods. Being air-breathers, nothing has apparently occurred to prevent their safe migration to other ground whenever that which they may have at any time occupied became uncongenial by reason of physical changes, because as a rule, those changes were effected so slowly that a continuity of congenial habitat for such mollusks was not necessarily broken. They were thus apparently as capable of preserving a continuous existence through successive geological periods as the marine mollusca were.

But, as before intimated, when we come to the study of the fossil shells of the fresh-water gill-bearing mollusca, which in their living state must necessarily have been confined to fluviatile and lacustrine waters, it is not easy to understand, without a special explanation, how continuous genetic lines could have been preserved (as we find they were preserved even down to the present time) through a succession of geological periods, during which the great lakes of the Laramie and succeeding Tertiary periods, as we know, and all the rivers which flowed into and from them, as is generally but erroneously believed, have been successively obliterated.* Rivers are separated from each other by intervening land, and, running to the sea, their mouths are separated by marine waters, neither of which barriers are fresh-water gill-bearing mollusks capable of passing. But if it can be shown that throughout those geological periods and down to the present time there has been direct continuity of fresh water by means of lakes or rivers, or both, the case is plain enough. Indeed, as precarious as the existence of continuous life of that kind may seem to have been, under the circumstances of such vast physical changes as are known to have occurred, we are forced to conclude that it is in this direction that we must seek for an explanation of the manner in which were preserved the fresh-water molluscan types that have been found in the various groups of North American Mesozoic and Cenozoic strata, and that we also find among living mollusca. That is, they have been preserved through a continuity of habitat in the congenial fresh waters of lakes and rivers, flourishing in the lakes, when they existed, as well as in the rivers, and escaping by the streams which were the former

* It may be suggested that the distribution of these forms from one river or river system to another, may have taken place by the transportation of the mollusks or their eggs by aquatic birds. While such transportation is admitted to have been possible in some cases, it cannot be admitted as a probable cause of any considerable part of the distribution that must have occurred during the several geological epochs in which the mulluscan types referred to are known to have existed. Notwithstanding the annual migration of myriads of aquatic birds between the northern and southern portions of North America ever since it has been a continent, the fresh-water molluscan faunæ of those regions, respectively, are still distinct.

outlets and inlets of the lakes, but which continued to flow after the obliteration of the latter, as rivers or tributaries of river systems.

Lakes are only parts of unfinished river systems which disappear when the system is finished by the erosion of its channel to a nearly uniform slope. A lake consequently contains essentially the same aquatic fauna that the fluviatile portion of the system does in case the water of the lake is wholly fresh; or a modification of that fauna if the water is more or less saline. The great lakes of the Tertiary and Laramie periods have successively become obliterated; but we may reasonably conclude that at least a part of the river channels of to-day have existed as such from former geological times; that the greater part of them were established in epochs anterior to our own, and that those of some of the tributaries of the present Mississippi River system are identical, at least in part, with former outlets or inlets, or both, of the great ancient lakes which have been referred to. Consequently we may reasonably conclude also that the molluscan fauna of the Mississippi River system is lineally descended from the faunæ of those ancient lakes, and the river systems of which they constituted lacustrine portions. This view is confirmed by the identity of the living with the fossil molluscan types which have been found so abundantly in those Laramie and Eocene deposits.

In these remarks the ancient Laramie sea is included under the term "lacustrine," the term "sea" being used simply to indicate that its waters were saline and not fresh; just as the Black and Caspian are called seas instead of lakes, and for the same reason. It may seem to be the use of a misnomer to speak of the Laramie sea as a part of a river system, because it was so immensely large, and the continental area which was drained into it was proportionally so small, but if these views concerning the conditions which then existed are correct, that sea, with its tributaries and outlet, differed only in degree and not in kind, from any river system which has a lake of any size in its principal course. The waters of that sea having been saline, the Laramie hydrographic system more nearly resembled that of the Black Sea than any other now existing that is equally well known; and, although that ancient sea has long since disappeared from the face of the earth, its "Hellespont" still flows as a part of the Missouri River, or of some one of its tributaries.

Rivers have of course existed ever since a sufficient extent of continental surface was raised above the sea to accumulate the waters that fell from the clouds; and in view of the mighty changes that have taken place during the progressive

growth of the North American continent, especially the elevation of its great mountain systems and plateaus, it would be natural to suppose that the earliest rivers at least have been obliterated. Some have no doubt been obliterated, but contrary to what has been the general belief, the recent labors of Powell, Dutton and others have shown that the rivers of North America have been among the most persistent of its physical features; that many of them are older than the mountain ranges of the regions which the rivers traverse, and that they have not yielded their "right of way" when the mountain ranges and plateaus were raised, but continued during and after that elevation to run in essentially the same lines which they had chosen when the region they traversed was a plain instead of a mountainous one. That ancient river systems have been in some and perhaps many instances, to a greater or less extent divided, as a consequence of unequal continental elevation, or from other causes, is quite certain; and it was doubtless in part by this means that the dispersion of freshwater mollusca into different river systems has been effected. That some formerly existing rivers have been obliterated and their molluscan faunæ destroyed is doubtless also true, but these facts do not necessarily affect the correctness of the view concerning the general persistent integrity of rivers and river systems which has been referred to.*

The coalescence of separate minor drainage systems by the confluence of their lower portions into a common channel during the progressive elevation of the continent has also been an important means of the dispersion of fluviatile Mollusca. By such coalescence, what were once separate rivers or minor drainage systems became parts of larger ones; as, for example, the union of the separate peripheral members of the great Mississippi River system, which now forms a common drainage for the principal part of the continent. The Ohio and Upper Mississippi, the two most ancient portions of the present great system, were once separate rivers, emptying into a northern extension of the Great Gulf; and it is practically certain that neither of them received that portion of the molluscan fauna, which now so strongly characterizes them, until after the confluence with them of the western portions of the

* The discovery of so few traces of fluviatile deposits as have been made among the strata of the earth is probably due to the persistent adherence of rivers to their ancient channels. When land upon which rivers have formerly run has subsided beneath the level of the sea, the fluviatile deposits were doubtless destroyed by the encroaching marine waters. If the land continued to rise, as has been so generally the case in the gradual production of the North American continent, the earlier river deposits were swept away in later times by their own waters, as their valleys were broadened and deepened. It is therefore, as a rule, only in the deposits of lacustrine portions of ancient river systems that their faunæ have been preserved.

present great river system which brought that fauna from its ancient home in the western part of the continent.

Rivers having been thus persistent, and the manner in which confluence of the waters of many of them has been effected being understood, it is no more remarkable that the types of fresh-water gill-bearing mollusca have come down to us practically unchanged, than it is that marine and land mollusks have reached us bearing the imprint of their really ancient, but what we have been accustomed to call, modern types.

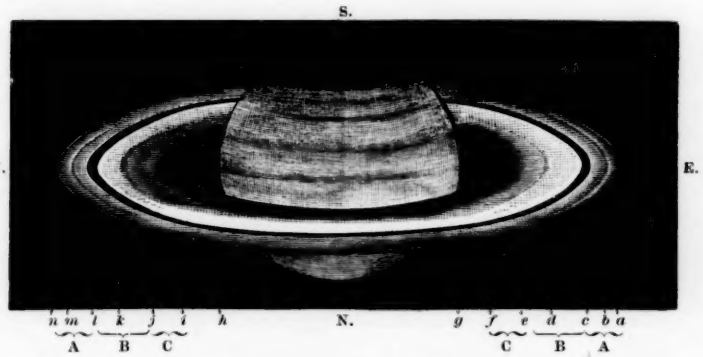
Not only have the molluscan denizens of the great Mississippi drainage system descended to their present habitat in the manner suggested; but there is no reason to doubt that a large part of the fishes of that system descended in the same manner, and in company with them. This is thought to be especially true of the characteristic Ganoids of that system. The progenitors of many of the fresh-water fishes may have ascended from the sea by the mouths of the rivers which have since coalesced to form that great river system; but it is believed that all did not do so.

Both molluscan and ichthyic life doubtless began in the sea; and it seems at least probable that the freshwater denizens of a large part of both these classes became such by compulsion rather than from choice. That is, their original progenitors were probably land-locked by the gradually and unequally rising portions of the sea-bottom which thus became portions of the respective continents; and they were compelled to either accustom themselves to the gradually freshening waters, from which they could not escape, or die. That many fresh-water forms have arisen by differentiation after their original progenitors were thus entrapped is evident; but this differentiation, at least among the Mollusca, has been slight and very slow, compared with what took place among the marine representatives of those classes during the same geological periods. Indeed a large number of the types among the Mollusca of the Mississippi drainage system have come down wholly unchanged from a time at least as remote as the Laramie Period.

ART. XLV.—*Measures of the Rings of Saturn in the years* 1879, 1880, 1881 and 1882; by EDWARD S. HOLDEN.

IN his memoir entitled *Recherches sur Saturne, ses anneaux et ses satellites* (1880), Mr. W. Meyer gives the results of his recent observations with the 10-inch equatorial of the Geneva Observatory.

Some of these results will not be received without criticism, since they go to show that the ball of Saturn is excentrically situated in the ring-system, and that the breadth of this system is not alike on the two sides. Specifically M. Meyer says, "je regarde donc comme établi par les observations qu' á l'époque où elles ont été faites, *la largeur de l'anneau de Saturne a été plus forte à son côté ouest, et que le centre même de la planète était plus près de l'extrémité est de l'anneau que l'autre.*" The results of his measures are given in table XI of his memoir, and in a table which accompanies the present note.



In order to make observations of this class readily comparable I have adopted a simple nomenclature which will cover all possible measures which can be made on Saturn. I have employed this in an extended examination of all the published drawings of Saturn known to me; and I take this opportunity of asking for a notice of the existence of rare drawings of Saturn, of those privately printed, and so forth. This system is shown in connection with a drawing of Saturn which was made at this observatory, 1881, November 27. The letters at the bottom of the drawing relate to points along the major axis of the ring.

a is the east end of the major axis.

b is the pencil line so-called (seen Nov. 27, 1881). It is too broad in the cut.

c is the *middle* of the principal division.

d is the point in ring B where the shading off begins; it is a definite point.

e is the outer edge of the dusky ring.

f is the inner edge of the dusky ring.

g is the east limb of the planet.

The remaining letters, *h* . . . . *n*, refer to the west half of the system.

Part.	W. Struve, 1826.	Bessel, 1830-3.	Encke, 1837-8.	Galle, 1837-8.	Main, 1840.
ab	----	----	----	----	----
ac	2·61	----	2·76	3·36	----
ae	6·71	----	7·39	7·27	6·55
af	----	----	----	----	----
an	40·095	39·311	40·93	40·90	38·33
ag	11·05	11·13	11·63	11·50	11·37
c (width)	0·41	----	----	----	----
cd	----	----	----	----	----
ce	4·11	----	4·63	3·91	----
cf	----	----	----	----	----
ci	----	----	----	----	----
cl	34·882	----	35·41	34·18	----
de	----	----	----	----	----
ef	----	----	----	----	----
eg	4·34	----	4·24	4·23	4·82
ej	26·67	----	26·15	26·36	25·24
fg	----	----	----	----	----
fi	----	----	----	----	----
fl	----	----	----	----	----
gh	17·991	17·053	17·68	17·91	15·60
hi	----	----	----	----	----
hj	4·34	----	4·24	4·23	4·82
hn	11·05	11·13	11·63	11·50	11·37
ij	----	----	----	----	----
il	----	----	----	----	----
in	----	----	----	----	----
jk	----	----	----	----	----
jl	4·11	----	4·63	3·91	----
jn	6·71	----	7·39	7·27	6·55
kl	----	----	----	----	----
l (width)	0·41	----	----	----	----
ln	2·61	----	2·76	3·36	----
mn	----	----	----	----	----
Polar diam.	----	15·381	16·49	----	----

It is unnecessary to remind anyone who has looked over the great number of papers on Saturn, that for the want of a simple system of referring to the planet's features we are often in doubt as to what points, measures, and especially descriptions, refer. So far, we have only the nomenclature of Struve who called the outer ring A, the inner B, and the dusky ring C.

The accompanying table contains about all the useful measures which have been made on this system since 1826.

In the last column are given those of M. Meyer, which are by far the most complete as to scope, and which are still in progress. From these it will be seen that his measures place the ball of Saturn excentrically in the rings, and that the width of the ring system is not alike on both sides.

Of course, such conclusions need confirmation, as they are contrary to some of the most complete series of observations which we have.

Part.	O. Struve, 1852.	Lassell, 1852.	Jacob, 1853.	De la Rue, 1854.	Secchi, † 1854-56.
ab	----	----	----	- -	----
ac	2·55	----	2·44	2·47	3·117
ae	7·42	----	6·80	6·46	7·590
af	9·46	----	8·87	----	9·738
an	39·73	40·881	39·91	39·83	40·893
ag	11·07	11·714	11·03	11·09	11·616
c (width)	----	----	0·37	----	----
cd	----	----	----	----	----
ce	4·87	----	4·36	3·99	4·473
cf	6·91	----	6·43	----	6·621
ci	27·74	----	28·61	----	28·040
cl	34·63	----	35·03	34·39	34·659
de	----	----	----	----	----
ef	2·04	[2·59]‡	2·07	----	2·148
eg	3·66	----	4·23	4·63	4·027
ej	24·90	----	26·32	26·91	25·714
fg	1·61	----	2·16	----	1·88
fi	20·83	----	22·19	----	21·419
fl	27·74	----	28·61	----	28·040
gh	17·59	17·453	17·86	17·66	17·661
hi	1·61	----	2·16	----	1·88
hj	3·66	----	4·23	4·63	4·027
hn	11·07	11·714	11·03	11·09	11·616
ij	2·04	[2·59]‡	2·07	----	2·148
il	6·91	----	6·43	----	6·621
in	9·46	----	8·87	----	9·738
jk	----	----	----	----	----
jl	4·87	----	4·36	3·99	4·473
jn	7,42	- -	6·80	6·46	7·590
kl	----	----	----	----	----
l (width)	----	----	0·37	----	----
ln	2·55	----	2·44	2·47	3·117
mn	----	----	----	----	----
Polar diam.	----	----	16·51	----	----

* Bond's results are given in terms of $an=1\cdot000$, and not in arc. To utilize them I have supposed $an=40''\cdot47$, by which they become directly comparable with Mayer's, the most complete series.

† All the measures previous to 1880 I have treated on the supposition that they give a symmetrical figure to the system. I believe that the original measures of Struve (1826) and of Secchi (1854) differ slightly from this supposition, but as I have not access at present to their original papers, I have borrowed the data from Kayser's Memoir in the Leyden Observations, vol. iii.

‡ Lassell in 1852 wrote to O. Σ. that $ij=0\cdot56\ hj$.

§ In Bond's later (unreduced) observations given in the same volume of the Harvard College Observatory Annals as his first, he usually estimates *ef* as less than one-half of *ge*.

I have made some measures of this system which are interesting in this connection, and which I shall give here although the series is not finished. They all relate to the position of the dusky ring C; and the position of the points *f* and *i* is invariably referred to (the *middle* of) the principal division, that is, to the points *c* and *l*. The observations were made in 1879 and 1880 with the 26-inch equatorial at Washington, and in 1881 and 1882 with the 15-inch equatorial here.

Part.	Bond,* 1848–56.	Jacob, 1856.	Kayser, 1862-3.	Meyer, 1880.
ab	----	----	----	----
ac	2·60	2·329	2·622	[3·00]
ae	6·96	6·863	5·806	6·97
af	9·31	8·892	----	9·88
an	40·47	39·997	39·471	40·47
ag	11·58	11·03	11·10	----
c (width)	0·47	----	----	----
cd	----	----	----	----
ce	4·36	4·534	3·184	[3·97]
cf	6·71	6·563	----	6·88
ci	28·57	28·777	----	[28·05]
cl	35·28	35·339	34·227	----
de	----	----	----	----
ef	2·35§	2·029	----	2·91
eg	4·62	4·166	5·292	4·45
ej	26·56	26·271	27·859	26·32
fg	2·27	2·14	----	----
fi	21·86	22·214	----	21·17
fl	28·57	28·777	----	27·58
gh	17·32	17·940	17·27	17·451
hi	2·27	2·14	----	----
hj	4·62	4·166	5·292	4·42
hn	11·58	11·03	11·10	----
ij	2·35§	2·029	----	2·24
il	6·71	6·563	----	6·42
in	9·31	8·892	----	9·42
jk	----	----	----	----
jl	4·36	4·534	3·184	4·18
jn	6·96	6·863	5·806	7·18
kl	----	----	----	----
l (width)	0·47	----	----	----
ln	2·60	2·329	2·622	3·00
mn	----	----	----	----
Polar diam.	15·46	----	15·392	16·021

These instruments are provided only with filar micrometers and are therefore not suitable for measuring diameters and distances such as occur in Saturn. This work properly belongs to the heliometer and the main part of it will undoubtedly soon be done at the Strassburg Observatory. But no heliometer now built has light enough to deal effectively with the dusky ring, and I therefore confined my measures with the Washington equatorial to that ring. The measures were referred

to the middle of the black division and not to some other point for obvious reasons.

All the distances were measured parallel to the major axis of the ring. In the Washington series they were single distances, each set usually containing four; at Madison all are double distances, four in each set.

I may add that it is my experience that the dusky ring is an object which requires to be *measured* under a high magnifying power, although it often seems to be better seen with a low one. As soon as measures commence, however, the advantages of the high power become apparent.

Observations of Saturn's Dusky Ring made with the 26-inch equatorial at Washington in 1879, *and reduced to* $\Delta = 9{\cdot}5389$.

No.	Date, 1879.	*if*	*ii*	*ci*	*cf*	Eye-piece	Wt.	*ci*—*if*= *cf*—*ii*=
			"		"			"
1	Sept. 10	----	6·88	----	7·02	400	4	+0·14
2	11	----	7·36	----	7·83	400	1	+0·47
3	16	----	6·83	----	7·45	400	2	+0·62
4	24	----	7·05	----	7·24	400	2	+0·19
5	25	----	7·31	----	7·69	400	4	+0·38
6	26	----	6·99	----	7·72	400	2	+0·73
7	29	----	7·07	----	7·58	600	4	+0·51
8	30	----	6·82	----	7·40	600	4	+0·58
9	Oct. 2	----	6·73	----	7·57	600	3	+0·84
10	5	----	7·12	----	7·65	600	4	+0·53
11	11	----	7·27	----	7·49	600	5	+0·22
12	14	----	6·86	----	7·63	600	3	+0·77
13	20	----	7·11	----	7·21	T.*	3	+0·10
	Means,	----	7·03 ± 0·04	----	7·50 ± 0·04			+0·47

Washington Observations of 1880 *reduced to* $\Delta = 9{\cdot}5389$.

No.	Date, 1880.	*if*	*ii*	*ci*	*cf*	Eye-piece	Wt.	*ci*—*if*= *cf*—*ii*=
		"		"				
14	Aug. 27	27·35	----	27·45	----	T.*	4	+0·10
15	27	27·62	----	27·58	----	400	4	−0·04
16	28	----	6·56	----	6·92	400	4	+0·26
17	Oct. 9	28·02	----	27·74	----	800	4	−0·28
18	10	27·44	----	27·74	----	800	4	+0·30
19	11	27·26	----	27·51	----	600	5	+0·25
	Means,	27·54 ± 0·09	6·56	27·60 ± 0·05	6·92			+0·10

* Terrestrial eyepiece magnifying 400 diameters.

Observations of Saturn's Dusky Ring made with the 15½-inch equatorial of the Washburn Observatory in 1881 and 1882, reduced to Δ=9·5389.

No.	Date, 1881.	*lf*	*li*	*ci*	*cf*	Eye-piece	Wt.	*ci*−*lf*= *cf*−*li*=
		"	"	"	"			"
20	Dec. 8	----	----	27·81	----	260	3	cloud'd
21	8	----	6·35	----	6·66	260	3	+0·31
22	9	----	6·34	----	6·55	430	3	+0·21
23	14	----	6·52	----	6·70	430	2	+0·18
24	17	----	6·62	----	6·80	430	2	+0·18
25	18	----	6·36	----	6·60	430	2	+0·24
26	23	----	6·47	----	7·02	430	5	+0·55
27	23	----	6·15	----	6·15	T.*	5	+0·00
28	26	----	6·52	----	6·78	750	4	+0·26
29	26	27·80	----	27·88	----	750	4	+0·08
30	28	28·35	----	28·25	----	750	1	−0·10
31	30	----	6·76	----	6·94	430	2	+0·18
32	31	----	6·83	----	6·82	430	4	−0·01
33	31	27·96	----	28·05	----	430	4	+0·09
	1882							
34	Jan. 2	27·96	----	27·98	----	430	4	+0·02
35	3	----	6·47	----	6·86	430	3	+0·39
36	11	28·15	----	28·26	----	430	4	+0·11
37	Feb. 6	27·89	----	28·03	----	260	3	+0·14
38	7	27·83	----	28·23	----	260	3	+0·40
	Means,	27·99 ± 0·050	6·49 ± 0·040	28·05 ± 0·041	6·63 ± 0·051			+0·18
	No. obs.	(7)	(11)	(8)	(11)			(18)

I can say that the Washington observations were made under good circumstances, with sufficient magnifying powers and present a satisfactory agreement among themselves. (Their accidental errors are materially less than those of the Geneva series.) Also the Madison observations are entirely satisfactory to me. The difference between the two series is so far, not explained. A constant difference between the width of the rings B and C west and east is uniformly shown in both the Geneva and the other series.

The greater precision of measures having *i* as a terminal point will be noticed and it agrees with remarks made in my observing books.

According to M. Mayer	1880;	$cf-li = +0''.46$
According to Washington	1879;	$= +0''.47$
According to Washington	1880;	$= +0''.10$
According to Madison	1881–2;	$= +0''.18$

It is somewhat remarkable that the four entirely independent series of observations agree in giving a value of *cf* materially larger than *li*. In the case of my own observations, I have

* Terrestrial eyepiece magnifying about 240 diameters. This power is too low for satisfactory measures.

been, and I am, somewhat skeptical as to the reality of this difference. In a system so complicated as that of *Saturn* constant errors may easily creep in. It was for that reason that I had constructed in Washington the terrestrial eye-piece which, unfortunately, I could use only on two nights. The terrestrial eye-piece for Madison is of too low a power for advantageous use, and I propose to replace it by one of higher power. By using this in connection with the regular astronomical eye-pieces, the existence of any personal difference which may exist between measures east and those west will show itself in the measures themselves, and I propose to carry these on until some satisfactory solution of these systematic differences is reached.

So far I have only considered the residuals *cf*—*li*, and they show a satisfactory agreement throughout my own three series and agree with those of M. Meyer. When we come to consider the absolute values of the things measured the discrepancies are relatively enormous. For example, *cf* in 1879 I found to be $7''{\cdot}50 \pm 0''{\cdot}04$ from thirteen nights, while in 1881 I found *cf* to be $6''{\cdot}63 \pm 0''{\cdot}051$ from eleven nights.

I can scarcely believe that even in these delicate measures I could make a constant error of nearly $1''$ in arc in the measure of a distance of $7''$. Nor can I accept the explanation that the distance *cf* has changed by $1''$ in this time; certainly not without further proof. My reason for presenting the question in this unsolved state is that by so doing M. Meyer's conclusions may be examined more closely by those possessing large telescopes, and as other measures appear to present the same anomalies, certainly I shall endeavor to present further evidence on this point.

I do not believe that the difference in the telescopes employed has anything material to do with the difference in results. The edges of ring C were well seen in both telescopes.

I add a few measures of the position of belts on *Saturn's* surface made in 1878. I did not continue these measures longer as no useful result seemed to be promised.

Washington measures of Belts on Saturn.

Date, 1878.	Wash. m. t.		A	B
	h	m		
Sept. 9	10	30	2·8	4·6
17	10	40	2·4	5·2
19	10	50	2·8	5·1
Oct. 1	11	30	2·4	5·2
8	10	50	2·8	--
14	9	30	2·2	5·1
21	8	30	2·6	--
25	9	18	2·6	4·7
26	9	36	1·8	4·3
Nov. 5	10	44	2·8	5·4::

N. B.—The measures in columns A and B are as follows: A=distances of the south edge of the bright equatorial belt of Saturn's southern hemisphere from the major axis of the ring; and B=distance of the center of the dark band in Saturn's northern hemisphere from the major axis of the ring.

I may add here that I have recently reduced the Washington observations of the position angle of the major axis of *Saturn's* ring and determined the inclination of the plane of *Saturn's* ring to the plane of the orbit of *Saturn*. The observations were made on seventy-seven nights in the years 1877, 1878 and 1879 which were favorable for this purpose. The 26-inch equatorial, the filar micrometer and eye-pieces magnifying from 400 to 800 times, were employed. The reductions have been made by Bessel's rigorous formulæ given in his *Abhandlungen*, vol. i, p. 321. The quantity sought is i', which is the inclination of the plane of *Saturn's* ring to the plane of *Saturn's* orbit; the results are as follow:

From the forty observations of Professor Hall we have for 1877·948=1877, Dec. 11; $i'=26°\ 38'\ 47''\pm1''.37$. The probable error of a single observation is $\pm8''.6$.

From the thirty-seven observations made by me we have for 1878·771=1878, Oct. 8; $i'=26°\ 57'\ 2''\pm1''.69$. The probable error of a single observation is $\pm10''.3$.

Bessel's twenty-two observations give 1818·726=1818, Sept. 20; $i'=27°\ 0'\ 9''\pm5''.2$. The probable error of a single observation was $\pm24''.1$.

I have not combined the results obtained by Professor Hall and myself since the observations of October 13, 14, 1877 and of November 13, 14, 1878, show that they are not comparable. My observations agree with those of Bessel within the limits of the probable errors. A comparison of the observations of Professor Hall with Bessel would indicate a diminution of the inclination since 1818.

Washburn Observatory, University of Wisconsin,
Madison, 1882, January 31.

ART. XLVI.—*Interference Phenomena in a new form of Refractometer;* by ALBERT A. MICHELSON.

IN an experiment undertaken with a view to detecting the relative motion of the earth and the luminiferous ether (Am. Journal of Science, No. 128, vol. xxii,) it was necessary to produce interference of two pencils of light, which had traversed paths at right angles with each other. This was accomplished as follows: The light from a lamp at *a*, fig. 1, was separated into two pencils at right angles, *bc*, *bd*, by the plane-parallel glass *b*, and these two pencils were returned to *b* by the mirrors *c* and *d*, whence they coincided along *be*, where they were viewed by the eye, or by a small telescope at *e*.

It is evident that, so far as the interference is concerned, the apparatus may be replaced by a film of air whose thickness is *bc–cd*, and whose angle is that formed by the image of *d* in *b*, with *c*.

The problem of interference in thin films has been studied by Feussner, but his equations do not appear to give the explanation of the phenomena observed. In particular, in the "Annalen der Physik und Chemie," No. 12, 1881, on page 558, Feussner draws the conclusion that the interference fringes are straight lines, whereas, in the above described apparatus they are in general curves: and there is but one case—that of the central fringe in white light—which is straight.

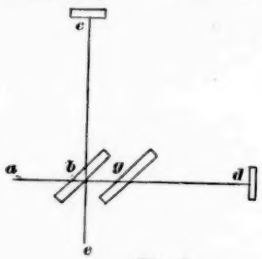


Fig. 1.

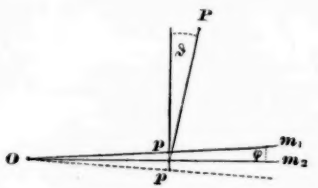


Fig. 2.

I have therefore thought it worth while to attempt the solution of the problem for a film of air, for small angles of incidence and neglecting successive reflections; and though the solution is not perhaps adapted to the general problem, it accounts for all the phenomena observed in the special case.

Let Om_1, Om_2, fig. 2, be two plane mirrors whose intersection is projected at O, and whose mutual inclination is φ. The illumination at any point, P (not necessarily in the plane of the figure), will depend on the mean difference of phase of all the pairs of rays starting from the source and reaching P, after

reflection from the mirrors; a pair of rays signifying two rays which have originated at the same point of the source.

If the area of the luminous surface is sufficiently large the illumination at P will be independent of the distance, form, or position of the surface. Suppose, therefore, that the luminous surface coincides with the surface Om_1. Its image in Om_1 will also coincide with Om_1, and its image in Om_2 will be a plane surface symmetrical with Om_1 with respect to the surface Om_2, and for every point, p, of the first image there is a corresponding point, p', of the second, symmetrically placed and in the same phase of vibration. Suppressing, now, the source of light and the mirrors, and replacing them by the two images, the effect on any point, P, is unaltered.

Consider now a pair of points pp'. Let ϑ be the angle formed by the line joining P and p (or p') with the normal to the surface; ϑ and φ being both supposed small,

$$\Delta = Pp' - Pp = pp' . \cos \vartheta .$$

The difference between this value of Δ and the true value is $2Pp . \sin^2 \frac{\psi}{2}$, where ψ is the angle subtended by pp' at P. If ϑ is a small quantity, ψ is a small quantity of the second order, and $\sin^2 \frac{\psi}{2}$ is a small quantity of the fourth order; consequently $2Pp \sin^2 \frac{\psi}{2}$ may be neglected. We have therefore, to a very close approximation, $\Delta = pp' \cos \vartheta$; or, substituting $2t$ for pp', $2t$ being the distance between the images, at the point where they are cut by the line Pp,

$$\Delta = 2t \cos \vartheta .$$

Let *cdef*, *c'd'e'f'*, fig. 3, represent the two images, and let their intersection be parallel with *cf*, and their inclination be

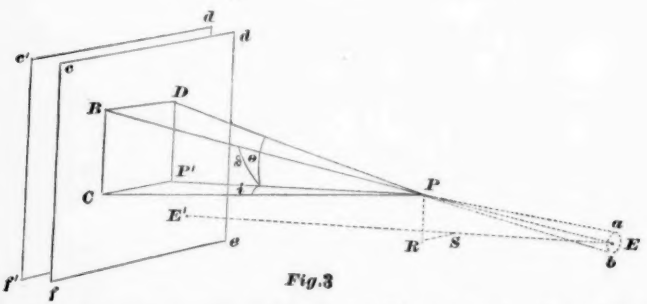


Fig. 3

2φ. Let P be the point considered; P', the projection of P on the surface *cdef;* and PB, the line forming with PP' the angle

ϑ. Draw P'D parallel to *cf*, and P'C, at right angles, and complete the rectangle BDP'C. Let $P'PC=i$ and $DPP'=\theta$. Let $PP'=P$, and the distance between the surfaces at $P'=2t_\varphi$. We have then

$$t=t_0+CP'.\tan\varphi=t_0+P\tan\varphi.\tan i, \text{ and}$$

$$\Delta=2(t_0+P\tan\varphi\tan i)\cos\vartheta, \text{ or}$$

$$\Delta=\frac{2(t_0+P\tan\varphi\tan i)}{\sqrt{1+\tan^2 i+\tan^2\theta}} \qquad (1)$$

We see that in general Δ has all possible values, and therefore all phenomena of interference would be obliterated. If, however, we observe the point P through a small aperture, *ab*, the pupil of the eye, for instance, the light which enters the eye from the surfaces will be limited to the small cone whose angle is bPa, and if the aperture be sufficiently small the *differences in* Δ may be reduced to any required degree.

It is proposed to find such a distance P, that with a given aperture these differences shall be as small as possible, which is equivalent to finding the distance from the mirrors at which the phenomena of interference are most distinct. The change of Δ for a change in θ, is

$$\frac{\delta\Delta}{\delta\theta}=-\frac{2(t_0+P\tan\varphi\tan i)\dfrac{\tan\theta}{\cos^2\theta}}{(1+\tan^2 i+\tan_2\theta)^{\frac{3}{2}}} \qquad (2)$$

The change of Δ for a change in i, is

$$\frac{\delta\Delta}{\delta i}=2\,\frac{(1+\tan^2 i+\tan^2\theta)\dfrac{P\tan\varphi}{\cos^2 i}-(t_0+P\tan\varphi\tan i)\dfrac{\tan i}{\cos^2 i}}{(1+\tan^2 i+\tan^2\theta)^{\frac{3}{2}}} \qquad (3)$$

For $\dfrac{\delta\Delta}{\delta\theta}=0$ we have $\theta=0$ (or $\Delta=0$).

For $\dfrac{\delta\Delta}{\delta i}=0$ we have $(1+\tan^2 i+\tan^2\theta)P\tan\varphi$

$$=(t_0+P\tan\varphi\tan i)\tan i, \text{ or}$$

$$(1+\tan^2\theta)P\tan\varphi=t_0\tan i, \text{ whence } P=\frac{t_0}{\tan\varphi}\tan i\cos^2\theta$$

Hence the fringes will be most distinct when $\theta=0$ and when

$$P=\frac{t_0}{\tan\varphi}\tan i \qquad (4)$$

This condition coincides nearly with that found by Feussner.

If the thickness of the film is zero, or if the angle of incidence is zero, the fringes are formed at the surface of the mirrors. If the film is of uniform thickness, the fringes appear at infinity. If at the same time $\varphi=0$, and $t_0=0$, or $i=0$ and $\varphi=0$, the position of the fringes is indeterminate. If i has the

same sign as φ, the fringes appear in front of the mirrors; if i has the opposite sign, the fringes appear behind the mirrors.

To find the form of the curves as viewed by the eye at E, let SE=D; call T the distances between the surfaces at E′, the projection of E. From P draw PR parallel to DP′, and RS at right angles, and let RS=c. We have then t_0=T+c tan φ, whence, substituting for c its value D tan i,

$$\Delta = 2\,\frac{\mathrm{T}+(\mathrm{D}+\mathrm{P})\tan\varphi\tan i}{\sqrt{1+\tan^2 i+\tan^2\theta}} \qquad (5)$$

If on a plane perpendicular to EE′ at distance D from E, we call x distances parallel to P′C, and y distances parallel to P′D, reckoned from the projection of E on this plane, then, putting tan φ=K and D+P=S, we have for the equation to the curves, as they would appear on this surface to an eye at E,

$$\Delta = 2\frac{\mathrm{DT}+\mathrm{SK}x}{\sqrt{\mathrm{D}^2+x^2+y^2}} \quad \text{or}$$

$$\Delta^2 y^2 = (4\mathrm{S}^2\mathrm{K}^2-\Delta^2)x^2+8\mathrm{TSKD}x+(4\mathrm{T}^2-\Delta^2)\mathrm{D}^2 \qquad (6)$$

If, numerically,

$\Delta < 2\mathrm{SK}$ the curve is a hyperbola,
$\Delta = 2\mathrm{SK}$ the curve is a parabola,
$\Delta > 2\mathrm{SK}$ the curve is an ellipse,
K=0 the curve is a circle,
$\Delta = 0$ the curve is a straight line,

All the deductions from equations (4) and (6) have been approximately verified by experiment.

It is to be observed that in the most important case, and that most likely to occur in practice, namely, in the case of the central fringe in white light, we have Δ=0, and therefore also t_0=0; and in this case the central fringe is a straight line formed on the surface of the mirrors. Practically, however, it is impossible to obtain a perfectly straight line, for the surface of the mirrors is never perfect.

It is to be noticed that the central fringe is black, for one of the pencils has experienced an external, the other an internal reflection from the surface b, fig. 1. This will not however be true unless the plate g (which is employed to compensate the effect of the plate b) is of exactly the same thickness as b, and placed parallel with b. When these conditions are not fulfilled, the true result is masked by the effect of "achromatism" investigated by Cornu (Comptes Rendus, vol. xciii, Nov. 21st, 1881). This remark leads naturally to the investigation of the effect of a plate of glass with plane parallel surfaces, interposed in the path of one of the pencils.

The effect is independent of the position of the glass plate, provided its surface is kept parallel with the corresponding mirror. Suppose, therefore, that it is in contact with the latter and let *cd*, fig. 4, represent the common surface. Let $t = hi$ = thickness of the glass, i=angle of incidence, r= angle of refraction, n=index of refraction, λ = wave-length of light. Let *ef* represent the image of the other mirror, and put $n_0 = \frac{hk}{t}$.

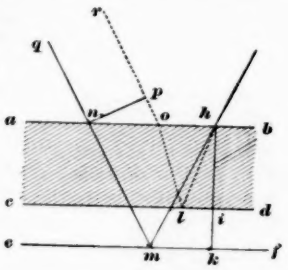


Fig. 4.

It can be readily demonstrated that the path of the rays in the instrument is equivalent to that given in the figure, where one of the rays follows the path *qnmh*, and the other the path *rolh*. Suppose the mirrors *cd* and *ef* parallel. Then as has been previously shown, the curves of interference are concentric circles, formed at an infinite distance. Therefore the rays *qn*, *ro*, whose path is to be traced, are parallel, and from the point *h* they coincide. Their difference of path is $2nm - 2hl - op$, and their difference of phase is

$$\varphi = \frac{2nm}{\lambda} - \frac{2hl}{\frac{\lambda}{n}} - \frac{op}{\lambda} = \frac{2n_0 . t}{\lambda \cos i} - \frac{2t . n}{\lambda \cos r}$$

$$- \frac{2t}{\lambda}(n_0 \tan i - \tan r) \sin i, \text{ whence}$$

$$\varphi = \frac{2t}{\lambda}[n_0 \cos i - n \cos r] \qquad (7)$$

Let it be proposed to find the value of n_0 which renders any particular ring achromatic. The condition of achromatism, as given by Cornu, is $\frac{d\varphi}{d\lambda} = 0$, which gives

$$\varphi + 2t\left(\cos r \frac{dn}{d\lambda} - n \sin r \frac{dr}{dn} \cdot \frac{dn}{d\lambda}\right) = 0.$$

We have $n = \frac{\sin i}{\sin r}$ whence $\frac{dr}{dn} = -\frac{\sin^2 r}{\sin i \cos r}$, whence

$$\varphi + \frac{2t}{\cos r} \cdot \frac{dn}{d\lambda} = 0$$

By Cauchy's formula we have $n = a_1 + \frac{a_2}{\lambda^2}$ whence $\frac{dn}{d\lambda} = -\frac{2a_2}{\lambda^3}$.

Substituting, we have $\varphi - \frac{4\alpha_2 t}{\lambda^3 \cos r} = 0$ or $n_0 \cos i = n \cos r = \frac{2\alpha_2}{\lambda^2 \cos r}$

$= \frac{\frac{2\alpha_2}{\lambda^2} + n \cos^2 r}{\cos i \cos r}$, or finally,

$$n_0 = \frac{2(n - \alpha_1) + n \cos^2 r}{\cos i \cos r}. \qquad (8)$$

If the angle i is small, the value of n_0 will vary very little with i, consequently there will be a large number of circles all nearly achromatised. Under favorable circumstances as many as one hundred rings have been counted, using an ordinary lamp, as source of light. The difference of path of the two pencils which produce these rings in white light may exceed a thousand wave lengths.

ART. XLVII. — *On two New Minerals, Monetite and Monite, with a notice of Pyroclasite;* by CHARLES UPHAM SHEPARD; *with analyses*, by C. U. SHEPARD, Jr.

THE specimens here described were sent to us by Mr. John G. Miller of Ottawa, to whom Canadian mineralogy owes so many mineralogical discoveries. They came from the Twin islands, Mona and Moneta, which are situated forty miles from the port of Mayaguez, Porto Rico, W. I.

1. *Monetite.*—The Moneta mineral, which we call monetite from its locality, is accompanied by two other species; all of them, however, have originated together in a guano-formation. The prevailing rock of the island is probably a Tertiary marine limestone, which at the top of the ground has been coated with bird-guano. The soluble ingredients of this investment, in percolating the highly porous strata, have been thrown down in their transit in the metamorphosed combinations now met with, lining the walls of cavities in the formation.

The material supplied came from one of these caverns. The specimens present a remarkable feature, as compared with the stone-guanos of the West Indies. The latter are usually uncrystalline, more or less compact, heterogeneous in their composition; and contain considerable traces of organic matter. They very rarely present dissociated simple minerals; but rather constitute what are called rock-aggregates. The examples from Mona, on the contrary, are almost wholly made up of distinctly separate mineral species, for the most part are crystallized, and as free from organic impregnation as if derived from trap or granite. The most abundant species in

the specimens received, is the monetite which sometimes forms isolated patches, half the size of one's hand. The second constituent of the masses is a snow-white gypsite, either crystallized, fibrous or pulverulent; while the remaining one is calcite, in well-formed, semi-transparent crystals. No alumina or oxide of iron is present in the aggregate.

The monetite, besides occurring in thick isolated masses as above mentioned, also forms irregular seams through the gypsite; and shows itself likewise in thick crusts, lining irregular shaped cavities. More rarely, it presents itself in botryoidal shapes, with rough crystalline surfaces. But under all circumstances, it is an highly crystalline mineral.

Mineralogical description.—Primary form,* right oblique angled prism. M on T about 143° as determined approximately by the common goniometer. Secondary forms, terminal and acute lateral edges replaced by single planes, the former generally very narrow. Height of prisms less than one-third their longest breadth; sometimes much less. Greatest length of crystals between $\frac{1}{12}$th and $\frac{1}{30}$th of an inch. They exhibit numerous irregular surface indentations, but are without striæ or curvatures. They cross and interpenetrate each other in several directions, some of the groups suggesting a regular composition of individuals. Though exhibiting frequent rifts, the cleavage is indeterminate. Fracture, uneven. Luster, vitreous. Semi-transparent. Color, pale yellowish white. Hardness =3·5. Gravity =2·75, which is a little below the actual, from the impossibility of wholly clearing the crystals of the mealy white monite and gypsite by which they are more or less coated.

Before the blowpipe, heated in a glass tube, turns white and evolves moisture, but unattended by odor. In the platinum forceps, turns white and melts into a globule with crystalline facets.

Analysis.

	I.	II.	Mean.
Lime	39·92	40·59	40·255
Phosphoric acid	44·41	49·79	47·100
Sulphuric acid	7·20	1·90	4·550
Water	8·47†	7·88	8·175
	100·00	100·16	100·080

4·55 per cent sulphuric acid calls for 3·185 per cent lime and 2·047 per cent water; thus constituting 9·782 per cent of gypsite. The mineral lost 0·2 per cent of moisture on drying it several hours at nearly 100° C. Subtracting the above constituents we have:

* See also the note on the crystalline form by E. S. Dana, on p. 405.

† The water in the first analysis was estimated by difference.

Phosphoric acid	47·100
Lime	37·070
Water	5·928
	90·098

And raising the above figures to 100, we have as the composition of the *monetite*,

Phosphoric acid	52·28
Lime	41·14
Water	6·58
	100·00

Dividing the above percentages by the respective molecular weights, we have:

Phosphoric acid	$52·28 \div 142 = 0·368$
Lime	$41·14 \div 56 = 0·735$
Water	$6·58 \div 18 = 0·366$

or very nearly, as 1 phosphoric acid: 2 lime: 1 water. This would require the formula $2CaO, H_2O, P_2O_5$ (or $CaHPO_4$), as a comparison of the calculated and obtained results will show.

	Calculated.	Found.
Phosphoric acid	52·20	52·28
Lime	41·18	41·14
Water	6·62	6·58
	100·00	100·00

Monetite is a crystalline dicalcic hydric-phosphate, or dicalcic-ortho-phosphate, differing from that artificially prepared (by the action of calcic chloride on disodic-hydric-phosphate), in not possessing water of crystallization as does the latter—$(PO_4H)_2Ca_2+2H_2O$.

2. *Monite.*—Intimately associated with the monetite, above described, is a hydrated tricalcic phosphate, resembling in color and density the more friable varieties of kaolinite, and which we propose to designate *monite*, after one of the islands where it is found. It has the following characters:

Massive, slightly coherent, impalpable and wholly uncrystalline; snow-white, fracture earthy, dull; hardness, below 2, gravity 2·1 (approximately). B.B. melts with difficulty to an opaque, white enamel of feeble luster. In closed tube emits much moisture.

Analyses.

				Mean.
P_2O_5	40·39	39·44	39·75	39·86
CaO	50·04	50·89	49·51	50·15
SO_3		2·57	1·75	2·16
H_2O			7·56	7·56
				99·73

Deducting

2·16 SO_3=1·51 CaO=0·97 H_2O=gypsite, 4·64

95·09

	Remaining.	Raised to 100.	Molecular weights.	Approximate ratios.
P_2O_5	39·86	41·92 ÷ 142 = 0·295		1
CaO	48·64	51·15 ÷ 56 = 0·913		3
H_2O	6·59	6·93 ÷ 18 = 0·385		$1\frac{1}{4}$
	95·09	100·00		

Or corresponding to $Ca_3P_2O_8+H_2O$ with some slight excess of moisture, probably hygrometric.

The two minerals, monite and monetite, are much intermingled, constituting together three-quarters of the variously sized masses, whose remaining quarter consists of gypsite and calcite, the latter however in isolated patches and in much the least proportion. The remarkable feature of the aggregate as coming from a stone-guano formation, consists in the entire absence (in masses, half a foot in thickness), of all traces of organic matter. The monetite is the most abundant species, sometimes occupying areas several inches long by one or two broad; and inasmuch as its crystals are confusedly aggregated and rather sharp, the specimens are exceedingly rough to the touch. The interspaces between the crystals occupy as much room as the crystals themselves; and are more or less filled with the monite. When the intervals are completely occupied with the latter, the aggregate is often arranged in imperfect layers, separated by intervals of about one-sixth of an inch, filled by the monite. A cross-fracture of such masses displays an obscurely banded appearance. Rarely the monetite presents globular concretions, with a sub-fibrous structure. It is also sometimes granular, but when pure it is rarely impalpable.

The associated gypsite is white, in small shining crystals, in coarse fibrous individuals, small globules, fine granular and pulverulent. The calcite is in distinct, semi-transparent crystals, having the form of acute rhomboids, analogous to those resulting from the slow evaporation of brine-waters. The presence of silica, or some insoluble silicate is detected in the analyses of the aggregate only, where it varies from 0·5 to 2· per cent.

Pyroclasite.—As the series of specimens sent were said to have been from caves, which abound at these islands, it would appear that their contents originated in percolations across the Tertiary strata. Indeed, examples of the lime-rock are among the collection, evincing the progress of metamorphosis from the unaltered calcic carbonate, to entire calcic phosphate. Wherever the solutions in their descent encountered caves or open joints in the strata, stalagmite and incrustations would result. Several specimens of these were among the collection sent. They strongly resemble the impure stalagmites of our limestone caves. Portions of the guano-stalagmite are singularly in the shape of mushrooms; and must have occupied the surface of the floor-crust. When broken, their structure is seen to be concentrically laminated, and not fibrous. In color they are brown, intermingled in spots with ash-gray, suggestive of a composite constitution. Other masses appear to have come from the floor stratum itself; and are hard and compact, with a subconchoidal fracture, like the monetite and like some varieties of opal. Its color is brown, though much darker and occasionally almost black. It is moreover variegated with ash-gray. The specific gravity is 2·62–2·65: hardness, 3·5–4·0. B.B. it decrepitates, emits a decidedly organic odor, turns white and fuses, but less easily than monetite, to a white, shining enamel. Heated in a glass tube, it decrepitates, with liberation of much water and strong empyreumatic odor. The subjoined analyses give its chemical composition.

	Robertson.		Shepard.	Mean results.
P_2O_5 (total)	40·81		38·80	39·080
CaO	40·27		39·98	40·125
SO_3	6·85		6·80	6·825
Phosphate iron and alumina			2·90	2·900
Insoluble	0·58		1·13	0·855
Water and loss on ignition	9·76		10·91	10·335
	98·27		100·52	100·120
Deduct gypsite		14·670		
" insoluble		0·855		
" phosphates iron and alumina		2·900		
" $\frac{1}{3}$ of loss by ignition, as organic		2·422		
				20·847
				79·273

This raised to 100 gives

			Calculation requires
P_2O_5	38·08	49·30 ÷ 142 = 0·347	49·05
CaO	35·35	44·59 ÷ 56 = 0·796	44·06
H_2O	4·84	6·11 ÷ 18 = 0·339	6·29
	79·27	100·00	100·00

Admitting the following formula:

$$3(Ca_2H_2P_2O_8)+Ca_3P_2O_8+H_2O.$$

The substance above analysed is undoubtedly identical with that from Monk's Island (Caribbean Sea) described in 1856, in this Journal, vol. xxii, p. 97, and named pyroclasite, from its very striking property of decrepitation when heated.* Whether it forms a true mineral species must depend upon more extended examinations. The analyses given favor the idea of its being an uniform compound of monetite and monite. It may, however, prove only a mechanical mixture of the two. Whether chemical or mechanical, it is eminently prone to admixture with gypsite, aluminum and iron phosphates, silica and organic matter, constituting the important phosphatic rock of the West Indies and South Carolina.

It may be added in conclusion that the collection embraced several specimens of antillite, which is identical with the trappean rock mentioned in connection with the pyroclasite of Monk's Island.

* An associated mineral which I then examined and called glaubapatite is essentially the same thing; the soda found therein having without doubt been owing to the damaged state of the cargo from which it was taken. The vessel that brought it to this place was bound for Baltimore; but was compelled, from damage at sea, to put into this port. The sulphuric acid found in the mineral was erroneously dosed with soda instead of lime. With the correction of these errors of analysis I desire to withdraw the name glaubapatite. The mineral to which it was given comes under pyroclasite.

Note on Crystals of Monetite.—The crystals of monetite placed in my hands may with tolerable certainty be referred to the triclinic system. The general form is that of a rather thin rhomboid. The longer lateral edge is replaced by the plane 100 (*a*), and the shorter by the hemi-prism $1\bar{1}0$ (*I*); there are also present in this zone the brachypinacoid 010 (*b*), and two other hemi-prisms *hk̄o* (*m* and *n*) between 100 and $1\bar{1}0$, and a third *hko* (*l*) on the other side of 100. The top of the tabular crystals is formed by the basal plane 001 (*c*) which is rough and uneven, and the edge behind, between *a* and *c*, is replaced by a dome $\bar{1}01$ (*e*). The angles measured with a reflecting goniometer are only roughly approximate, and would not justify an attempt to calculate the axial ratios and inclinations. The supplement angles are: *aI* ($100 \wedge 1\bar{1}0$)=42°, *ab* ($100 \wedge 0\bar{1}0$)=81°, *am*=17°, *an*=28°, *al*=18°, *ac*=76°, *ae* ($100 \wedge \bar{1}01$)=138°. There appears to be a distinct cleavage parallel to *a*. The crystals often interpenetrate each other, forming complex groups, but there is no uniform law of composition.—E. S. Dana.

Charleston, Feb. 18, 1882.

ART. XLVIII.—*Notice of the remarkable Marine Fauna occupying the outer banks off the Southern Coast of New England, No. 6*; by A. E. VERRILL. (Brief Contributions to Zoology from the Museum of Yale College: No. LII.)

Parasmilia Lymani Pourtales.—*Remarkable modes of growth and repair.*

POURTALES mentions that in this cup-coral the young bud out from the inside of the parent-calicles and burst them, by their normal growth. So that, afterwards, they remain attached to fragments of their own parents. This is probably an erroneous explanation of facts identical with some of those that I have observed. We have dredged this coral in considerable numbers, and of larger size than those described and figured by Pourtales. In each locality nearly all the specimens are attached, at base, to the inner surface of fragments, more or less old and discolored, of the same coral, as observed by Pourtales. Moreover, we have been able to collect a series sufficient to fully demonstrate that all these cases are instances of buds arising from the inner or septal surfaces of such specimens as have been accidentally broken or crushed. The coral is very fragile and has a tendency to split longitudinally into wedge-shaped segments when it breaks. Each fragment then has the power of developing from its still living internal tissues, and especially from the membrane (endoderm) covering the septa, one or more buds, which then grow up into cornucopia-shaped cups, like the parent. In many specimens a single large bud will start from near the distal end of a large piece; such a bud will grow up into a regular, more or less curved, circular or elliptical cup, with a thick base, continuous in part with the original fragment; the costæ and septa of the old fragment are perfectly coincident with the costæ and septa of the new coral on the outer or marginal side; examples of this kind, in all stages of development, were taken; one of these buds is 14^{mm} broad at base and 17^{mm} high on the proximal side, 7^{mm} high on the distal side; another is 13^{mm} broad at base and 23^{mm} high; another is 10^{mm} broad at base, 22^{mm} high, the circular calicle 14^{mm} broad; another is 6^{mm} broad at base, 44^{mm} high, the elliptical calicles 20 by 25^{mm}. In several cases two buds starting close together, from a single fragment, or from two imperfectly separated pieces, have grown together more or less completely, so as to produce a double calicle, the two halves either equal or unequal, and separated by a constriction, but with two septal centers and columellæ, and two distinct mouths. The largest example of this kind is 50^{mm}

high, the two calicles together, 40^{mm} across, each one about 20^{mm} broad. In one instance the base of a good-sized cup has been broken off, where about 6^{mm} in diameter; from the broken and ragged ends of the septa thus exposed three buds have started out, each 3 to 4^{mm} broad, with 24 to 36 septa; the old calicle appears to have been unaffected. In another instance a calicle that had developed, as usual, from a fragment, after it had grown 22^{mm} high was itself badly injured, so that half of the interior of the calicle died, but was not broken, but from the other half a large bud arose, nearly filling the old calicle by its new growth, and soon becoming larger than before the injury, so that on one side the cup is nearly regular and the costæ continues to the base, while on the other side the edge of the old calicle, with the old septa in place, appears in high relief, at about mid-height of the cup; in this case we have, as it were, three generations shown in one specimen; this example is 45^{mm} high, the calicle 25^{mm} broad. One of the most remarkable examples is a large cup, 50^{mm} high and 28^{mm} broad, in which the inner margin and distal portion of the septa are discolored, indicating that the soft parts within the edge of the calicle, and probably the tentacles, disk and stomach had been destroyed by access of mud, or some other accident, while the parts farther within the deep cup, or at least the membranes there covering the walls and septa, remained alive; from the edges and surfaces of the septa over thirty small buds have started out, varying in size and degree of development; the greater number form a partial circle, 10 to 12^{mm} from the margin, but others are scattered over the more interior parts of the cup, even to very near its base, the columella being nearly absent; the larger of these buds are about 5^{mm} in diameter, but some are not more than 2^{mm}, and many are just beginning to appear, and have slight walls and septa only on one side. In all cases the still living edges of the old septa serve as some of the septa of the new growth.

A number of large specimens were dredged, off Chesapeake Bay, in 57 fathoms (station 899) by Lieut. Z. L. Tanner, on the "Fish Hawk," Oct., 1880. It was dredged by our party, on the "Fish Hawk," off Martha's Vineyard, in 100 to 130 fathoms (stations 940, 949 and 1040), in 1881. It was more common and largest at station 940, in 130 fathoms. Off Florida,—Pourtales.

In speaking of ***Flabellum Goodei*** (p. 313), I have mentioned that it also has this same mode of budding from the inner surface of the fragments. Some of these phenomena have been noticed in other deep-sea corals, though not explained in the same way. Probably this will, hereafter, be found to be a common mode of increase in the case of various fragile cup-

corals. Indeed, I believe that the same explanation will apply to some of the ancient cyathophylloid corals, in which buds arise within and appear to destroy and supplant the parent calicle. Instead of such buds being the *cause* of the *death* of the parent-polyp, as has been supposed, I believe them to be due to the *result* of injury and the consequent effort of the tissues to *retain life* and repair injuries.

Restoration of the disk in Ophiurans.

That Ophiurans restore their rays with remarkable facility when broken, or entirely lost, is well known. In examining a large series of *Amphiura abdita* V., collected in the harbor at Noank, Conn., among eel-grass (*Zostera*), in 1874, I found several specimens in which the entire dorsal disk, with the contained viscera, had been lost and more or less restored, showing the various stages of the process. The dorsal disk of this species is soft and swollen, and is very easily detached. The arms are exceedingly long and slender and subject to frequent restorations. In some of the examples in which a new disk was forming, the scars are still plainly visible, on the bases of the arms, showing where the disk had been torn away, and its former size. In some of these the new disk, though perfect in form, had not grown to more than one-third, or one-half the diameter of the old one; in others it was nearly completed. These small disks connected with the full sized arms and jaws of the adult, give such specimens a very peculiar appearance. At first I mistook some of these for the genuine young.* But a more careful examination easily revealed their true nature.

In the same lot were specimens in which a portion of the edge of the disk, with one or two of the arms, had been destroyed, and afterwards restored. In a few instances two arms had grown out, in place of one.

* It is quite probable that *Amphiura macilenta* V., described in the February number of this Journal, p. 142, is really the true young of *A. abdita*. There are no *genuine young* of the shallow-water form known to me. The restored specimens, with small disks, appeared so different that, until I discovered their true nature, the identity of the two forms did not seem to me possible.

ERRATA.

Page 309, line 26, for *Ballicina*, read *Balticina*.
Page 313, line 7 from bottom, for S. O. Sars, read G. O. Sars.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the determination of Gas-densities.*—Goldschmidt and Victor Meyer, having occasion to make a series of determinations of the density of cyanogen gas at various temperatures, contrived for this purpose a simple method. The gas vessel is filled first with pure dry air at the temperature at which the determination is to be made. Then this air is displaced by a current of hydrogen chloride gas, collected over water and measured. Next the hydrogen chloride is displaced by air. After this the gas to be examined is passed through until all the air is replaced by it. This gas is again displaced by hydrogen or air and collected in a potash bulb filled with a liquid which absorbs it completely. The increase of the weight of the bulb gives the weight of the gas. The weight of the vessel full of air and of gas being now known for the given temperature the density is easily obtained. The apparatus consists of a cylinder of glass 200^{mm} high, 30^{mm} diameter, with capillary tubes attached to the ends, rising considerably above the upper end of the cylinder and there bent each at right angles. This vessel is heated in a glass tube slightly bulbous at bottom, 400^{mm} high and about twice the diameter of the inner tube, containing the liquid whose boiling point is the temperature of measurement. Water, aniline, amyl benzoate and diphenylamine are used in the glass outer vessel; but at temperatures of boiling sulphur or phosphoric sulphide, the inner vessel is spherical and the outer one of iron. Carbon dioxide and hydrogen chloride gases gave in this apparatus densities of 1·53 and 1·26 respectively. The apparatus may obviously be used as an air thermometer; and the authors determined the boiling point of sulphur as 426° C.—*Ber. Berl. Chem. Ges.*, xv, 137, Feb., 1882.

G. F. B.

2. *On the diffusion of Solids into Solids.*—Colson has observed that if an iron plate is heated in lampblack not only the carbon penetrates into the iron, converting it into steel and then into cast iron, but also considerable quantities of iron diffuse into the carbon. If the heating be sufficiently prolonged, this diffusion may be shown to take place even at 250°. If a piece of piano wire, imbedded in lampblack, be heated to redness in the reducing flame, it loses weight. Platinum under these circumstances shows no change. Since platinum does not combine with carbon, it appears that a diffusion occurs between solids only when they can react on one another. Pure silver loses weight when heated in pure dry alkali-chloride. But the product darkens on exposure to light; hence silver chloride must have been formed, free alkali having been produced by the oxygen of the air. If a polished piece of artificial iron sulphide be heated on a plate of copper in a current of CO_2 small quantities of sulphur go from the iron to the

copper. If a piano wire be heated in a crucible lined with carbon and filled with lime, the wire increases in weight and shows on analysis the presence of calcium.—*C. R.*, xciii, 1074. G. F. B.

3. *On the production of Active Oxygen.*—TRAUBE has summed up the conclusions upon the production of active oxygen to which he has been led by his experiments. He finds (1) that hydrogen-palladium, agitated with water and oxygen (or air) produces immediately and abundantly hydrogen peroxide; (2) that the oxidizing action of hydrogen-palladium in presence of oxygen and water, is not a direct one but depends almost entirely upon the hydrogen peroxide thereby produced; (3) that only in a single case has he observed the oxidizing action of hydrogen-palladium to be different from that of hydrogen peroxide; iodide of potassium and starch, which is not blued by H_2O_2, is blued at once by the hydrogen-palladium and oxygen; evidently by the carrier-action of the palladium transferring oxygen from the hydrogen peroxide to the potassium iodide; (4) that the experiments prove positively, in opposition to the view of Hoppe-Seyler, that nascent hydrogen cannot produce active oxygen from ordinary oxygen by splitting its molecule; and (5) that the frequent production of hydrogen peroxide in processes of oxidation is no evidence of the simultaneous presence of active oxygen, because in opposition to the common view, the peroxide is never produced by the oxidation of water by active oxygen. It is a reducing action which produces it even when the process in general is one of oxidation. Thus for example when zinc is agitated with water and oxygen, there is produced, beside zinc hydrate, also hydrogen peroxide. But no active oxygen is formed, the molecule of oxygen not being split, but the molecule of water, its oxygen combining with the zinc to form the hydrate, its hydrogen uniting directly with the oxygen molecule to form hydrogen peroxide, thus:

$$Zn+(H_2O)_2+O_2=Zn(OH)_2+\begin{matrix}O-H\\ |\\ O-H\end{matrix}.$$

Hydrogen peroxide is produced by the union of a molecule of oxygen with two atoms of hydrogen. By analogy with other bodies produced by reduction, *i. e.*, assumption of hydrogen, it might be called reduced oxygen. It behaves toward common oxygen as indigo-white to indigo-blue.—*Ber. Berl. Chem. Ges.*, xv, 222, Feb., 1882. G. F. B.

4. *On the so-called Persulphuric oxide of Berthelot.*—Since by the mode of its production as well as by its reactions and properties, the body discovered by Berthelot, and called by him persulphuric oxide, resembles the peroxides, such as H_2O_2, BaO_2, etc., MENDELEJEFF prefers the name sulphur peroxide for it. The bodies MnO_2 and PbO_2, which form salts, he would call dioxides only. The blue Cr_2O_5, the highest stage of oxidation of chromium must be viewed as chromium peroxide, or chromyl peroxide. According to the author's view, an element, according to its character, gives either basic or acidic oxides which possess the property of forming salts corresponding to the water type, and accord-

ing to which the so-called valence of the elements or their position in the system, is determined. Only after the production of the oxide forming the highest salts, can the element yield a peroxide of the type of hydrogen peroxide. There is no ground for the assumption that hydrogen, barium, etc., can give still higher stages of oxidation, and then perhaps yield acidic oxides. Sulphur peroxide confirms this view completely and leads to the expectation of the discovery of similar peculiar peroxides with many of the other elements.—*Ber. Berl. Chem. Ges.*, xv, 242, Feb., 1882.

G. F. B.

5. *On the production and properties of metallic Cæsium.*—SETTERBERG has worked up the tons of alums obtained in Marquart's laboratory as a by-product in the preparation of lithium salts from lepidolite, with a view to obtain larger quantities of the salts of cæsium and rubidium and if possible metallic cæsium itself. Three or four hundred weight of the alums were dissolved in so much water that at the boiling temperature, the liquid marked 20° B. The solution was decanted and allowed to stand 12 to 14 hours. The mother liquid contained no trace of cæsium or rubidium salts but the crystals were rich in these metals; the author having found that each of the different alums is insoluble in saturated solutions of the more soluble ones. Hence so long as the solution of the alums was saturated with potassium alum it contained scarcely a trace of the other alums; and the solution showed no trace of cæsium, so long as it was saturated with rubidium alum. By repeating this process the alums were obtained pure. Search for other alkali-metals gave a negative result. In 14 days, Setterberg prepared 40 kilograms rubidium alum and 10 kilograms cæsium alum both pure. At 17° C. 100 parts of water dissolved 1·42 parts of rubidium alum and 0·38 parts of cæsium alum. For the preparation of other salts, the alums were decomposed with barium hydrate, and the filtrate neutralized with the acid whose salt was desired. In this way the acid tartrates and the cyanides were prepared. For the preparation of metallic rubidium, 1500 grams hydrogen-rubidium tartrate, 150 grams calcium carbonate and the required quantity of sugar were mixed and calcined and the mixture transferred to a mercury flask and distilled. The yield was very satisfactory. A kilogram of hydrogen-cæsium tartrate similarly treated afforded no result. The electrolytic method was then employed first with the chloride and then with the cyanide of cæsium. Finally a mixture of 4 molecules of cæsium cyanide and one of barium cyanide was found to give a satisfactory result, the metal prepared showing in the spectroscope only a trace of sodium as an impurity. Cæsium resembles closely the other alkali-metals. It is silver-white, malleable and very soft at ordinary temperatures. Thrown on water it bursts into flame, and swims about on the surface like potassium and rubidium. It inflames in the air when not protected. It fuses about 26·5°, passing through a pasty condition. Its specific gravity is 1·88 at 15° C.—*Liebig's Ann.*, ccxi, 100, Jan., 1882.

G. F. B.

6. *On the metals of the rarer Earths.*—The position of the cerite metals in the periodic series of Mendelejeff has been for a long time a subject of discussion. BRAVNER has made a special study of these elements with reference to this point. With cerium he succeeded in preparing the tetrafluoride CeF_4, H_2O, as an amorphous brown powder, and a double fluoride $(KF)_2(CeF_3)_2(H_2O)_2$, as a yellowish white crystalline powder. With didymium, he prepared the hydrated pentoxide Di_2O_5, H_2O, and also the anhydrous pentoxide. The atomic weight of didymium was determined as 146·18 and that of lanthanum as 139. Hence the author places cerium (atomic weight 141·6) in the fourth group, eighth series, didymium in the fifth group, eighth series, and lanthanum in the third group, eighth series. This places Ce with C, Si, Ti, Zr, Sn, Pb and Th; and didymium with N, P, V, As, Cb, Sb and Bi.—*J. Chem. Soc.*, xli, 68, Feb., 1882. G. F. B.

7. *On the preparation of Cymene from Turpentine.*—NAUDIN has pointed out a reaction by which cymene can be prepared from turpentine with great facility. If two atoms of dry chlorine are absorbed by one molecule of turpentine cooled to −15°, there is no sensible evolution of hydrogen chloride but the liquid becomes viscous and contains $C_{10}H_{16}Cl_2$. A slight elevation of temperature produces decomposition, and cymene and hydrogen chloride distill together. If to the mixture 4 per cent. of phosphorous chloride be added, and the temperature be maintained at 25°, a regular evolution of HCl takes place until the conversion is complete. Washing with water, drying over calcium chloride and rectification over sodium, gives pure cymene boiling at 175°, the yield being 75 per cent. The author has observed that at 100° traces of zinc dust violently decompose the body $C_{10}H_{16}Cl_2$.—*Bull. Soc. Ch.*, II, xxxvii, 110, Feb., 1882. G. F. B.

8. *On a new Alkaloid from Cinchona Bark.*—In investigating the alkaloids of the variety of cinchona bark described by Flückiger as *China cuprea*, HOWARD and HODGKIN have obtained an alkaloid which appears peculiar to that species. This bark has recently been shipped in immense quantities from Buccarramanga in the province of Santander, where this tree is said to form whole forests. It yields from none up to 2 per cent of quinine, and small amounts of quinidine and cinchonine. The new alkaloid is in its properties analogous to quinine, having a composition nearly identical and nearly the same specific rotary power. It differs however in the solubility of its salts and in the readiness with which it crystallizes from ether. The authors propose for it the name *homoquinine*. The alkaloid itself dissolves very sparingly in ether, 100^{cc} dissolving only 0·57 at 12°. Alcohol of 90 per cent dissolves 7·64 parts. The sulphate resembles that of quinine, but the crystals are shorter and appear to contain 6 molecules of water. One part of this salt requires more than 100 of water for solution. In a 5 per cent solution of 90 per cent alcohol the alkaloid shows a rotation of −158°.—*J. Chem. Soc.*, xli, 66, Feb., 1882.

G. F. B.

9. *On the Coloring matter of the Chinese yellow berries.*—FOERSTER has examined the Chinese yellow berries used in dyeing (the buds of *Sophora japonica*). The glucoside obtained when treated with sulphuric acid yielded 46·84 per cent of a yellow substance and 57·56 per cent of isodulcite. This yellow substance was not quercetin however; and so the glucoside is not quercetrin. The author proposes for it the name sophorin. The glucoside of the caper (*Capparis spinosa*) and of the garden rue (*Ruta graveolens*) are apparently identical with quercetrin, yielding quercetin and isodulcite when decomposed by means of dilute sulphuric acid.—*Ber. Berl. Chem Ges.*, XV, 214, Feb., 1882. G. F. B.

10. *Maximum of magnetization of diamagnetic and weakly paramagnetic bodies.*—The researches of Hankel, Rowland and others have shown that iron, cobalt and nickel have a maximum point of magnetization. Silow has also maintained that a solution of chloride of iron possesses a maximum point. H. W. EATON, in this paper discusses the various results of different observers: concludes that a solution of chloride of iron does not possess a maximum of magnetization, but increases in permeability in proportion to the strength of magnetic field (he used from 1 to 7 Bunsen cells), and doubts whether the use of 30 Bunsen cells would show a maximum. Shumeister investigated the maximum conditions of water, alcohol, sulphide of carbon, ether, oxygen and nitrogen, and discovered apparently a maximum. Eaton repeats Shumeister's observations and maintains that this maximum condition does not exist in ordinary magnetic fields. Then follows a comparison of the observations of different observers which discloses great want of agreement. This discrepancy extends also to the various determinations of the diamagnetic constant of bismuth. The following table of the results of various observers is given:

		10^6 k.
1.	Weber (through magnetic influence)	−2·304
2.	" (through electrical influence)	−2·122
3.	Christie	−1·485
4.	Toepler and Ettingshausen	−1·492
5.	Rowland and Jacques (in C. G. S. units) { K′	−0·012554
	{ K″	−0·14324

K′ principal axis of crystal vertical.
K″ " " " horizontal.

—*Ann. der Physik und Chemie*, No. 2, 1882, pp. 225–246. J. T.

11. *Reflection of electrical rays.*—W. Hittorf has maintained that the rectilinear electrical rays proceeding from the kathode in a Geissler tube end when they infringe upon an immovable wall or barrier and are not reflected. E. Goldstein describes what he thinks is an *Experimentum Crucis*, and concludes that a bundle of kathode rays do not end but can excite phosphorescence by reflection from a solid partition or wall. Every surface in suitable condition can serve as a reflector—it matters not whether it is a phosphorescence exciter, an insulator, or a conductor. The

reflected rays can excite phosphorescence and are acted upon by a magnet like the original kathode rays.—*Ann. der Physik und Chemie*, No. 2, 1882, pp. 246–254. J. T.

12. *Influence of mechanical hardening upon the magnetic properties of steel and iron.*—Many investigations upon the relation between the molecular conditions of iron and steel produced by heat, by torsion and by annealing processes, and the resulting changes in magnetic conditions have been made. It appears from the paper of Louis M. Cheesman that the effect of mechanical hardening has not been properly investigated, and this paper contains the results of his investigation upon this point. The method of research consisted simply in determining the magnetic moment of the magnetic bar after it had been subjected to well devised mechanical pressures. The result of his investigations is summed up as follows: Iron in a mechanically hard condition can receive more permanent magnetism than in a soft condition. The magnetic moment of a steel magnet in a mechanically hard condition is greater or smaller than in a soft condition, according as the ratio of its diameter to its length is less or greater than a certain limit.—*Ann. der Physik und Chemie*, No. 2, 1882, pp. 204–225. J. T.

13. *Storage of Electricity.*—A commission consisting of Mm. Allard, L. LeBlanc, Joubert, Potier and Tresca, experimented upon Faure's battery in Paris, January, 1882. The Faure battery was composed of 35 elements, the lead plates of spiral form weighing each, with included liquid, 43·700^{kg}. The lead electrodes were covered with minium to the amount of 10^{kg} per square meter. The liquid consisted of distilled water with the addition of one-tenth of its weight of pure sulphuric acid. The charging machine was of the Siemens type the armature having a resistance of 0·27 ohms, and the inductor 19·45 ohms. The current of discharge was passed through a series of Maxim's incandescent lamps. The authors state, in general, that they obtained the light of one carcel with an expenditure of 5·80^{kgm} of electrical work per second. They were also led to the conclusion that it is advantageous to charge the battery with the feeblest current possible and to prolong the duration of the charge. The results of the investigation are summed up as follows: The charge of the battery demanded a total mechanical work equivalent to 1·558-horse power during 22^{h} 45^{m}, or 1-horse power during 35^{h} 26^{m}. The battery received in reality only 0·66 of this work, the rest having been dissipated in the work of excitation.

The exterior electrical work during the entire duration of the discharge amounted to 3809000^{kgm}; the mechanical work consumed was 9570000^{kgm}, but of this amount furnished only 6382000^{kgm} was retained by the battery. Hence the amount recovered during the discharge was 0·40 of the total work, and 0·60 of the stored-up work. The employment therefore of the accumulator has cost 0·40 of the work furnished by the dynamo-electric machine which might have been utilized in other ways. The

advantages of having a reservoir of electricity, however, compensates for this loss of energy.—*Comptes Rendus*, March 6, 1882, pp. 600–604. J. T.

14. *Storage of Electricity.*—Professor Ayrton, F.R.S., delivered a lecture on the storage of energy at the London Institution, March 2, in which he maintains, allowing for the unnecessary waste due to the too hurried charging and too hurried discharging of a Faure accumulator, "that for a million foot-pounds of stored energy discharged with a mean current of 17 ampères, the loss in charging and discharging combined need not exceed 18 per cent," and in some cases he has found it not to exceed 10 per cent. He has found from experiment that 81 pounds of lead and red lead charged and discharged, the discharge lasting eighteen hours—six hours on three successive days—represented in its discharge 1,440,000 foot pounds of work, or 1-horse power for three-quarters of an hour.

The resuscitating power of the Faure accumulator was wonderful. After the eighteen hours of discharge just referred to it was found after a few hours of insulation that the accumulator could give a current of over 50 ampères. The phenomenon resembles the invigorating action of sleep.—*Nature*, March 23, 1882. J. T.

15. *Velocity of Sound in Wood;* by Dr. H. Kayser. (Communicated).—In this Journal for February, 1879, Dr. Magnus C. Ihlseng has published a method of determining the velocity of sound in wood. I take this opportunity of making some remarks in regard to it, although at a somewhat late date. Mr. Ihlseng defines the velocity of sound in wood in two ways: in the first, the rod of wood is fixed in the middle, one end is rubbed and the other end marks its vibrations on a blackened glass plate; by this means are found the number of vibrations n, and half the wave length in wood, $\frac{\lambda}{2}$, equal to the length of the rod; then the velocity is $v=n\lambda$. The number of vibrations can also be found by Kundt's method, the end of the rod causing the air in a tube to vibrate; if $\frac{l}{2}$ designates half the wave length of air in the tube, a the velocity of sound in the tube, then $n=\frac{a}{l}$, and therefore $v=\frac{a\lambda}{l}$.

Mr. Ihlseng obtained in every case greater velocities by the second method than by the first, and was unable to find the reason for the difference. I think the explanation is easily given, and it is this which induces me to send this note. Unfortunately Mr. Ihlseng gives the results of his researches so very incompletely, that I cannot be sure that my supposition is right; but from the words of the author, I must presume, that calculating by the second method, he has taken for a the velocity of sound in free air, that is to say about $332{\cdot}5\frac{m}{\text{sec.}}$. This would be allowa-

ble only if his tube were ∞ wide, or the pitch ∞ high. If this is not the case there is a diminution of the velocity by friction and by conduction of heat, which diminution is to be calculated according to Kirchhoff* by the equation

$$u-a=\frac{u\gamma}{2r\sqrt{\pi n}}$$

where u means the velocity in free air, a the velocity in the tube, n the pitch of the tune, $2r$ the diameter of the tube, and γ a constant. This equation I have verified† by experiments and have determined the constant γ to be for glass tubes

$$\gamma=0{\cdot}0235\frac{m}{\text{sec.}}$$

On a later occasion‡ this number was again confirmed.

We shall see what numbers would have been obtained by Mr. Ihlseng with the second method, had he kept in view this equation. He indicates the diameter to be $2r=0{\cdot}019m$; the pitch n can be calculated nearly by his Table IV. I find by it numbers lying between 1000 and 2000; taking as average 1500 vibrations, we find

$$u-a=6m \text{ and } a=326{\cdot}5m.$$

Mr. Ihlseng having, therefore, calculated the velocity of wood by means of the dust figures by $v=\frac{332{\cdot}5\lambda}{l}$, the right equation would have been $v=326{\cdot}5\frac{\lambda}{l}$.

It follows, that for the mean velocity of $4000\frac{m}{\text{sec.}}$ in wood, the number calculated by Kundt's method is too great by $73m$. The mean difference between the numbers found by the two methods, according to Mr. Ihlseng is $61m$,—surely a sufficient agreement, considering that the probable error of his determination reaches $19m$.

Finally I remark, that according to my experiments on glass tubes, the graphic method is quite unfit for precise determinations, because, as Mr. Ihlseng also finds, the rod vibrates not only longitudinally, but at the same time transversely, and therefore the number of vibrations can be determined only very inexactly by the curves.

Berlin Physical. Institut., March, 1882.

16. *The Chemistry of Cooking and Cleaning, a Manual for Housekeepers;* by Ellen H. Richards. 90 pp. 12mo. Boston, 1882 (Estes and Lauriat).—The practical applications of chemical principles to some of the familiar processes of every-day household life have never been better presented than in this little book by Mrs. Richards. Clear and concise in style, and happy in its illus-

* Kirchhoff, Pogg. Ann., cxxxvii, p. 177. † Kayser, Wied. Ann., ii, p. 218.
‡ Kayser, Wied. Ann., viii, p. 444.

trations, it is a successful combination of good science with sound practical advice. It is to be hoped that the day is not far distant when every housekeeper will have the knowledge to understand these principles and the wisdom to apply them in all the homely operations which she has to direct or perform. This little book cannot fail to do good service in bringing about this desired end.

II. Geology and Natural History.

1. *Bulletin of the Illinois State Museum of Natural History* (at Springfield, Ill.). No. 1. Feb., 1882. 44 pp. 8vo. Printed for the Museum, 1882.—This first Bulletin of the Illinois State Museum contains three articles, (1) Descriptions of 54 new species from the Lower Carboniferous limestones and Coal Measures of Illinois and Iowa, and (2) Corrections and proposed new names for species previously described in the Geological Survey of Illinois under names that were preoccupied, and Descriptions of two new species of fossil shells from the Coal Measures of Illinois and Kansas, by A. H. Worthen, State Geologist, and (3) Descriptions of two new species of Crinoids from the Chester limestone and Coal Measures of Illinois, by Charles Wachsmuth. Mr. Worthen announces that all the new species described by him will be fully illustrated in the *Seventh* volume of the Geological Survey of Illinois, now in course of preparation. A large part of them are represented by specimens of unusual perfection partly in the State Collection, but largely in that of Mr. L. A. Cox, of Keokuk, Iowa. The changes of names of species in the published volume of the Illinois Geological Survey, given in the second paper by Mr. Worthen are the following: *Platyostoma Graycillensis* in place of *P. tumida*, vol. ii; *Modiolopsis rectiformis* for *M. orthonota*, vol. iii; *Mod. Carrollensis* for *M. subnasuta*, vol. iii; *Platyceras subsinuosa* for *Pl. subundatum*, vol. iii; *Pleurotomaria coniformis* for *Pl. conoideus*, vol. v; *Orthoceras Randolphensis* for *O. annulo-costatum*, vol. vi. *A. Lithophaga*, in vol. iii, p. 536, is here named *L. Illinoiensis*.

2. *Glacial-era Climate.*—M. Woeikof, in a paper on "Gletscher und Eiszeiten in ihrem Verhältnisse zum Klima," rejects the idea that an epoch of maximum eccentricity with aphelion in the winter would be favorable for a glacial era. He observes that in the interior of Asia, for example, the greater cold in winter would not favor a large accumulation of snow, while the excessive heat of the summer would produce the melting of the snows to a greater height than now. In the region of the monsoons, the cold and dry monsoons of the winter would have greater force than at present and be less snow-producing; while the great heat of summer would produce increased rains and melting to a higher level. The surface of the ocean would not be of a lower temperature than now, and hence there would be no increased fall of snow over the lands adjoining. The effects from maximum eccentricity

and the earth in aphelion would in any case be slight, and rather unfavorable than favorable to the production of a glacial era.

3. *Post-Glacial Flood.*—An article by H. H. HOWORTH, on the "Great Post-Glacial Flood," attributing to it the formation of the löss besides other deposits, is contained in the Geological Magazine, numbers for January and February, 1882.

4. *Quincy Syenite.*— Mr. M. E. WADSWORTH, in the Proceedings of the Boston Society of Natural History, vol. xxi, Oct. 19, 1881, describes the junction of the Paradoxides slate of Quincy with the syenite of the region, and concludes that the latter is eruptive and therefore of subsequent age to the slate—"late Primordial or more recent." The syenite is stated to be at the junction a "spherulitic quartz porphyry."

5. *An old chapter of the Geological Record, with a new interpretation: or Rock-metamorphism and its resultant imitations of organisms; with an introduction giving an Annotated History of the controversy on the so-called "Eozoon Canadense;"* by Professor W. KING and T. H. ROWNEY. 142 pp. 8vo, with 8 colored plates. London, 1881. (John Van Voorst.) —The authors have here given a catalogue of all papers on the Eozoon with critical notes, and a full statement of their own views on the subject of a branch of metamorphism with which the origin of the supposed fossil is associated.

6. *Species of Eurypterus and Pterygotus from the Water-lime group near Buffalo.*—Mr. JULIUS POHLMAN describes and figures the following as new species of these genera in the Bull. Buffalo Soc. Nat. Sci., vol. iv, No. 2, 1882: *Eurypterus giganteus, Pterygotus globicaudatus, Pt. acuticaudatus, Pt. quadricaudatus, Pt. Buffaloensis.*

7. *Chatauqua Scientific Diagrams, Series No.* 1. *Geology: and First Lessons in Geology;* by A. S. PACKARD, JR. 128 pp. 8vo, to accompany the diagrams. Providence, R. I., 1882 (Providence Lithograph Co.)—The geological diagrams, ten in number, are very large lithographic plates, coarse in style of execution, for use in the illustration of lectures or first lessons in geology. They contain restorations of some of the species of the several geological ages, and illustrations also of scenery and volcanic and glacial phenomena. They might be much better. Professor Packard's work treats briefly and in a popular way of the topics illustrated, the action of water and heat, and the succession of life on the globe, with special reference to North America.

8. *Tables for the Determination, Description and Classification of Minerals;* by JAMES C. FOYE. 85 pp. 12mo. Chicago, 1882 (Jansen, McClurg & Co.).—A second edition of Professor Foye's useful tables for the determination of minerals has been recently published; it contains some additional tables which will increase its value.

9. *On the Female Flowers of the Coniferæ.*—Under this heading Professor EICHLER of Berlin gives his latest views about this important subject in the Monatsberichte der K. Acad. der Wiss.,

Berlin, 1881. He takes it for granted that the morphologists of the present day are agreed that the male *flowers* are the aggregate of stamens which were formerly (and still by Parlatore in DeC. Prodromus) considered an ament, i. e. an inflorescence. He now tries to prove that what we call the female ament is perfectly conformed and corresponding in every respect to the male flower in position as well as in structure, and thus is also a single flower and not an inflorescence.

The simplicity of such an arrangement, the uniformity in the structure of the male and female organs thus established, are certainly seductive, and in the author's opinion the difficulties in his way are readily enough removed. The principal difficulty is found in the presence of two foliaceous organs in the female flower, one, now called the ovule-bearing or carpellary scale, inside of another, the bract, while the male flower consists of a number of simple pollen-bearing bracts (the stamens) arranged around the axis. Now, according to Professor Eichler's view, the carpellary scale is not a distinct organ, but really only an appendage, a ventral excrescence, a ligule, if it may be called so, of a leaf (the bract), born from it and belonging to it; he therefore recognizes only one organ, the bract, and the so-called carpellary scale as its appendage—a view already indicated by Sachs. He reviews and controverts the views of preceding morphologists. Robert Brown declared the carpellary scale to be a leaf in the axil of the bract; but this is a morphological impossibility. Schleiden and Strasburger took it for a flattened axis; but the arrangement of the bundles of vessels makes that untenable. Van Tieghem thought it was a leaf median on an undeveloped axillary bud, but Coniferæ never do produce such median leaves. A. Braun, and after him Caspary, were the first to recognize in the scale a compound organ, consisting of two lateral leaves of such an undeveloped bud, connate on their lower side; Oersted, and more decidedly Mohl, showed that these two leaves were connate with their posterior edges, turning their backs toward the main axis, and Mohl happily compared the scale thus constituted with the double leaf of Sciadopitys; this, however, seems impossible to Professor Eichler, because nothing is *seen* of such an assumed undeveloped axillary bud, and because the distribution of the vessels in the scale does not indicate a double origin, as it does distinctly in the Sciadopitys leaf.

Professor Eichler insists that the carpellary scale of Coniferæ, though often apparently separated from the bract, is really not a distinct organ, but, as stated above, a ligular appendage of the upper face of the bract. He shows that in such appendages the vascular bundles are always arranged in a system opposite to that of the leaf from which they originate; if they spring from the upper side of the leaf, this upper side is turned toward the upper side of the leaf, and if they come from the dorsal side, their dorsal side is turned toward the back of the leaf, and in either case the arrangement of the vascular elements is reversed. In every

leaf the xylem (wood-cells) occupies the upper or ventral side of the vascular bundle, and the phloëm (formerly bast or soft bast-cells) lies below it or toward the dorsal side of the leaf. This is so in the coniferous leaf as in all others, and in the bract, while the ligule or carpellary scale shows the arrangement reversed, a well established fact. This carpellary scale wherever it exists (principally in Abietineæ) bears on its back one or several ovules. In Araucaria it is a small process, of ligular form, from the middle of the leaf-bract, with one ovule; in Cunninghamia it is a narrow transverse crest with three ovules; in Cryptomeria it is a jagged crest, at last much larger than the bract itself; but in Damara no trace of an appendage can be found. The same is the case with most Taxodineæ, where only a certain thickening of the bract is noticeable. But in Abietineæ we see two apparently distinct organs; examining them, however, more closely, we find them always cohering at base, and where they at maturity separate from the axis as in Abies and Cedrus, they fall off together, as they should do, where one is only an appendage of the other. In Cupressineæ no carpel-bearing appendage at all is observed, and the ovules are as nearly axillary as possible, or, where there are many, they seem to spring partly from the base of the bract and partly from the axis itself; but the thickened upper side of the scale shows also a reversed arrangement of the vessels, "as it can not be otherwise, where the leaf is so thickened on the inner side that at last it even becomes peltate." The other Coniferæ follow the same rule, the ovules stand in the axil of the bract; but in Taxineæ they are terminal products, surrounded by the uppermost leaves or bracts.

The proliferation sometimes observed in the female flowers of Abietineæ has been thought to furnish proof for the axillary nature and distinctness of the carpel; but Professor Eichler is satisfied that the abnormally developed branchlet is not the development of a supposed carpellary axis, but a really new axillary production of the bract therefore between the carpellary appendage and the main axis, and that the division of the carpel into two pieces, often noticed in such monstrosities, is produced by pressure only.

Eichler then compares the ovule of the Coniferæ to the sporangium of allied vascular cryptogams, and finds in Isoetes the sporangium on the base of the leaf where also a ligula is present, in Lycopodium in the axils, and in Psilotum at the end of branchlets. The essential character of gymnospermy, however, he finds not as much in the open carpel, which also occurs in some higher organized plants, as in the absence of a stigma and in the immediate action of the pollen on the ovule.

In a second paper, "*Ueber Bildungsabweichungen bei Fichtenzapfen*" (Sitzungsb. d. K. Ac. Wiss., Berlin, 1882), Professor Eichler gives us a careful analysis of monstrosities observed on the Norway Spruce, and of one on the Himalaya Hemlock, and tries to show how former interpreters had misunderstood their teaching, and how these examples fully confirm his own views,

expressed in the foregoing paper. He has procured the very specimens which Stenzel, Willkomm, Oersted and Parlatore had examined, analyzed them with great care, and gives figures and sections of the principal forms found. He demonstrates that the so-called fruit scale in those monstrous cones is not divided into two lateral leaves, but becomes irregularly toothed, folded or lobed, sometimes simulating two or more leaves, and finally developes a knob and at last a bud, which may and does sometimes grow out into a branch, always on the inside of and adnate to the metamorphosed scale, i. e. on the side towards the axis of the cone, often enveloped by its folds, but never between it and the bract, which ought to be the case if the now prevailing view of the nature of the scale (as formed by two lateral leaves connate on their axial side) were true.

Unfortunately he has not had occasion to examine such monstrosities where these two leaves are foliaceous, partly or entirely distinct, originating not from the base but from the very axil of the bract, and not divided by any possible pressure, such as occur at the base, not at the top, of proliferous cones of *Tsuga Canadensis*. In such cones the transformation progresses from a pair of separate leaves in the axil of a leaf-like bract to the partly united, then to the small and notched scale, at last to the large ovuliferous scale in the axil of a small retuse bract.

It may be stated in passing, that the first lateral bracts at the base of a shoot, in pines at least, are *not* inclined outward or toward the supporting bract, but rather decidedly inward, toward the axis, and are overlapping on the inner and upper side of the shoot, leaf-bundle, or flower.

Professor Eichler has given us a very valuable contribution towards the solution of the interesting question he treats of; but he has not yet settled it, and it will continue to tax the ingenuity of morphologists. G. ENGELMANN.

10. *Domestication of Wild Ducks.* (Bulletin of the Buffalo Society of Natural Sciences, vol. iv, No. 2.)—In a paper "on the domestication of some of our Wild Ducks," Mr. Charles Linden, the author, states after efforts to domesticate several of the species, capturing them young or raising them from eggs, that none of those transferred to the barn-yard "adapted themselves thoroughly to this state excepting the Mallard, Dusky Duck and Canada Goose, the progeny of which prospered well and attained a greater weight and size than the ordinary domesticated stock. Some of them are still living and betray in many instances a tendency to revert in point of plumage to their original condition, while the majority have become completely metamorphosed into ordinary barn-yard fowl. No hybrids from any two different wild species, which bred only within the enclosure, were ever obtained excepting from crosses between the Mallard and Dusky Duck." The crossing was readily accomplished "without any need of resorting to special inducements." He says, "it is evident that the Dusky Duck is fully as domesticable as the Mallard

which has been thus far supposed to be the originator of our common tamed ducks."

11. *Conchologische Mittheilungen, als Fortsetzung den Novitates Conchologicæ, herausgegeben von* Dr. E. von MARTIUS, Professor in Berlin. Band I. 104 pp. 8vo, with 18 colored plates. Kassel, Germany, 1881. (Theodor Fischer.)—The plates illustrate species (with their varieties) of the genera *Nanina*, *Helix*, *Bulimus*, *Pleurotomaria*, *Pleurotoma*, *Buccinum*, *Pupa*, *Helicarion*, *Limnæa*, *Tornatellina*, *Stenogyra*, and *Partula*.

III. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

First Annual Report of the Bureau of Ethnology, to the Secretary of the Smithsonian Institution 1879–'80; by J. W. POWELL, Director. 604 pp. Roy. 8vo, with many plates and wood-cut illustrations. Washington, 1881.—After a prefatory statement, by the Director, of the work now in progress, under the auspices of the Ethnological Bureau, by a number of special investigators, this volume presents a series of very valuable papers pertaining chiefly to the North American Indians. The subjects of Mr. Powell's own reports are (1) the Evolution of language, (2) sketch of the Mythology of the North American Indians, (3) Wyandot government, (4) on Limitations to the use of some anthropologic data. These are followed by other reports as follows: a further contribution to the study of the Mortuary customs of the North American Indians, by H. C. YARROW; Studies in Central American Picture writing, by E. S. HOLDEN; Cessions of land by Indian tribes to the United States, by C. C. ROYCE; Sign language among North American Indians, by Col. GARRICK MALLERY; Catalogue of linguistic manuscripts in the library of the Bureau of Ethnology, by J. C. PILLING; Illustrations of the method of recording Indian languages, from the MSS. of J. O. DORSEY, A. S. GATSCHET and S. R. RIGGS. The numerous illustrations of the volume are highly instructive as to Indian rites, modes of life, hyeroglyphics, and sign language, and other topics discussed.

OBITUARY.

CHARLES ROBERT DARWIN, whose "Origin of Species" caused a change in the world's thought almost as abrupt as a turn in the tide, and whose later works have successively led the way into new fields of research, died on Wednesday, the 19th of April. Darwin was born on the 12th of February, 1809.

E. DESOR, of Neufchatel, Switzerland, died early in March. Mr. Desor was with Agassiz in his investigations of the Alpine glaciers, and later published an independent volume on the subject. He was the author also of various geological and paleontological memoirs. His property he bequeathed to the city of Neufchatel for scientific purposes.

Bulletin of the Museum of Comparative Zoology at Harvard College. Vol. IX. No. 1. "Description Sommaire des espèces nouvelles d'Astéries," by Edmond Perrier, Prof. Jardin des Plantes de Paris: being one of the Reports of the results of dredging under the supervision of Alexander Agassiz, in the Gulf of Mexico, 1877–78, by the United States Coast Survey Steamer "Blake." 32 pp., 8vo. Cambridge, 1881.